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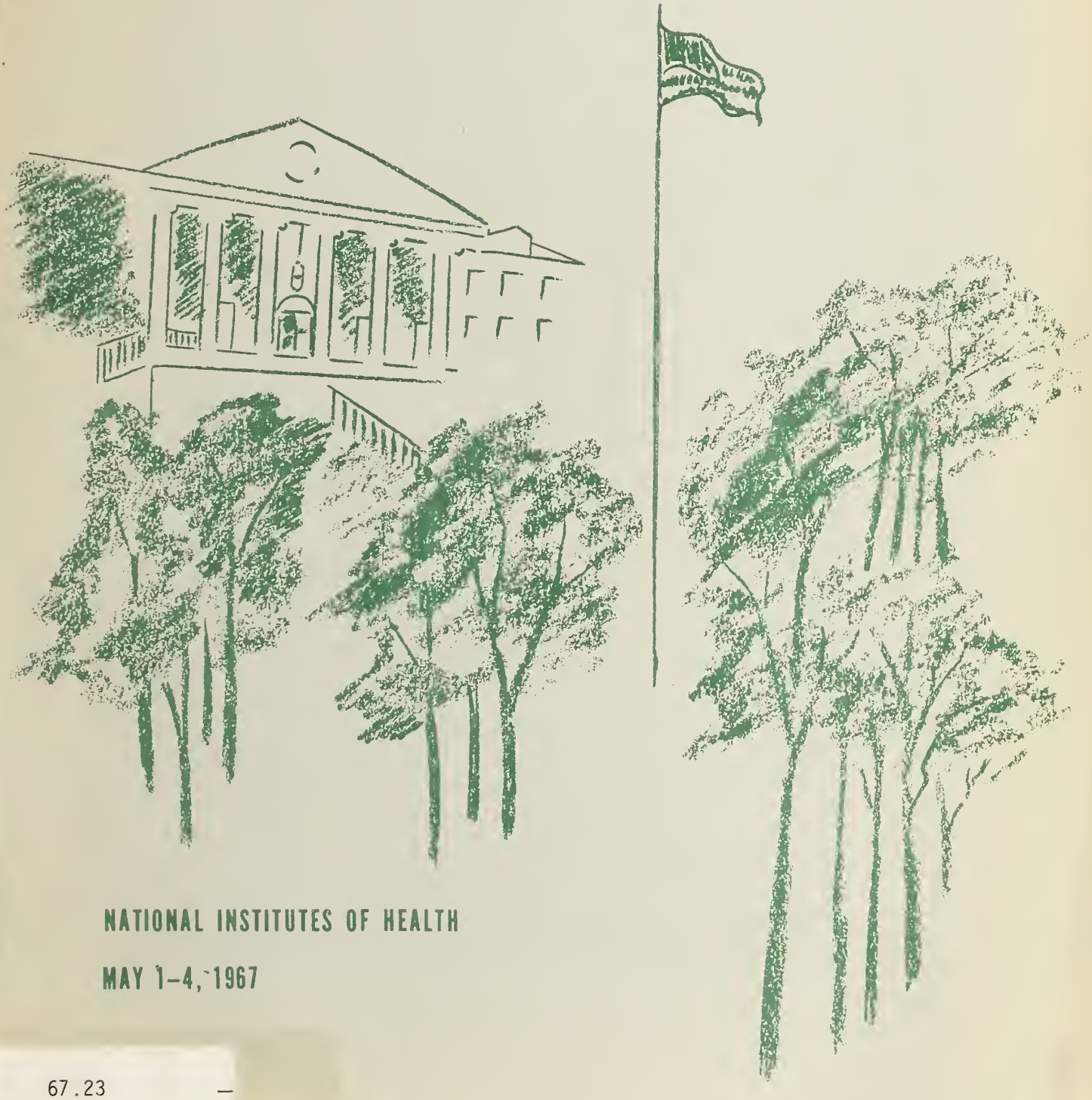
SEMINAR ON SCIENCE & PUBLIC POLICY

FOR SENIOR EXTRAMURAL STAFF

SUPPLEMENTARY READING



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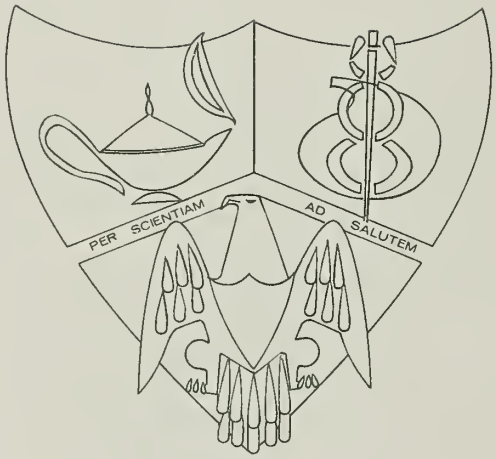
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Explanatory Note

Twice each year the Deputy Director for Extramural Programs, through the Committee on Staff Training--Extramural Programs, sponsors a Seminar in Science and Public Policy for the senior extramural staff of the National Institutes of Health. Reprints of significant published articles and papers are distributed to the members for their study and perusal in advance of the seminar. As issues change and as provocative and informative contributions to the literature appear, there are additions and deletions in the preparatory reading list.

The articles that follow are surplus copies of reading required for recent sessions of the Science and Public Policy seminar. It is probable that they will not be used in the future, either because other material will have been substituted, program content will have changed, or technical considerations (blurred reproduction, unsuitable format, etc.) preclude these particular pages from being incorporated into the reading collection. These articles should be of interest, however, to the NIH scientist-administrator who has not seen them. For this reason, they have been prepared in the present form for distribution to seminar members wishing to read beyond the assigned literature.

For further information about the seminar please refer to the inside of the back cover.

* * * * *

Sixth NIH Seminar on
Science and Public Policy

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"Conference on the Economics of Medical Research," from Report to the President-- The President's Commission on Heart Disease, Cancer, and Stroke, Vol. II, Washington, D. C., Feb. 1965, 631-643.

"Onward the Management of Science: The Wooldridge Report," Joseph D. Cooper, Science, 11 June 1965, 1433-1439.

"The Support of Science in the U. S.," Dael Wolfle, Scientific American, July 1965, 19-25.

"Funding and Managing an Accelerated Program of Applied Biomedical Research," William D. Carey, an address before the Conference on Research in the Service of Man, Oklahoma City, October 24, 1966.

"Criteria for Scientific Choice II: The Two Cultures," Alvin M. Weinberg, Minerva, Winter 1965, 3-14.

"Criteria for Scientific Choice," Alvin M. Weinberg, an address before the U. S. Civil Service Commission Institute for Career Science-Executives, National Institutes of Health, October 1962.

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"Science and Congress," Kenneth Kofmehl, an address before the Midwest Conference of Political Scientists, Madison, Wisconsin, May 2, 1964.

"The Fiscal Dilemma of Academic Science," William V. Consolazio, Bulletin of the Atomic Scientists, February 1965, 15-18.

"Manpower in the Technological Revolution," L. V. Berkner, Civil Service Journal, Jan.-March 1963, 12-25.

"The Scientist as Public Administrator," Lewis C. Mainzer, Western Political Quarterly, December 1963, 814-829.

"Viewpoint of the Scientific Community," W. N. Hubbard, Public Health Reports, August 1965, 677-680.

"The Management of Science in the Public Interest," Chalmers W. Sherwin, Bulletin of the Atomic Scientists, January 1964, 9-12.

"The Competition for Quality /Part II/," Federal Council for Science and Technology, Washington, D. C., April 1962.

*This list is accurate for the first 25 copies of this compilation. The remaining 25 contain fewer articles and some listed here are omitted because the supply was exhausted.

THE PRESIDENT'S SCIENCE ADVISERS

By
PHILIP H. ABELSON

Reprinted from
MINERVA
Winter 1965

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THE PRESIDENT'S SCIENCE ADVISERS

PHILIP H. ABELSON

The Role of the Adviser

TODAY and in the foreseeable future, a nation's prosperity and security depend upon the wisdom and timeliness of decisions made with respect to science and technology. The circumstances dictate that the best scientific and technical minds be consulted and that the results of their deliberations be conveyed promptly to the highest authority. Since the head of a government has a wide spectrum of responsibilities, can work closely with only a limited number of persons and is himself no expert in science and technology, it is not uncommon to maintain the post of Science Adviser and to use its incumbent as the primary source of counsel in scientific and technological matters. In the United States the President, since late 1957, has had frequent recourse to the services of a Science Adviser.

As with all human arrangements for the exercise of power there are advantages and disadvantages in concentrating authority for decision-making in one person. The office and its supporting machinery are relatively new and correctable deficiencies are to be expected. In what follows I will largely portray some negative features of the advisory complex.[1] My negative approach is due in part to the fact that it is easier for an outsider to identify failures than to specify successes. There is a paucity of positive evidence concerning the quality of performance of the President's advisers, especially the last two. The major tangible basis is a relatively thin collection of reports arising from studies conducted by panels appointed by the adviser.

As an illustration of the difficulty of identifying contributions of the Science Advisers, consider events surrounding the Nuclear Test Ban Treaty. Dr. Wiesner, who was Science Adviser at the time, was known to favour such a treaty. However, he was one among many proponents. In the important Senate hearings some 40 witnesses testified. He was not among them. Nor did he participate in the crucial negotiations at Moscow.

There is little public evidence that the adviser or his staff have addressed themselves to many major problems which might be expected to fall within the adviser's responsibility. For instance, a crucial problem is how to

[1] For a favourable treatment of the adviser's role, see special report on "White House Superstructure for Science", *Chemical and Engineering News*, XLII (19 October, 1964), 42, pp. 78–92.

harness effectively the vast scientific and technological resources of the nation. Do the present allocations of money and manpower make sense? If this is too big a question, an alternative one is: do present policies of research support at times damage the national interest? Some of us believe they do.

The advisers and their staffs have concerned themselves with many relatively trivial problems to the annoyance of some of the other parts of the government. Anyone acquainted with Washington civil servants engaged in science can learn of their discontent with the advisory system. Many such scientists have felt arrogantly mistreated by the men who have occupied the post of Science Adviser or by their agents. In what follows I will discuss the power structure available to the President's Science Adviser. I will then describe how the structure has at times functioned and make some suggestions for improvements.

The Science Adviser's authority stems from many sources. He is the President's personal science adviser, the Chairman of the President's Science Advisory Committee, the Director of the Office of Science and Technology, and the Chairman of the Federal Council on Science and Technology.

A major element in the power structure is the Office of Science and Technology. The formal objectives of the creation of this organisation can best be described by quoting directly from the President's Message to Congress of 29 March, 1962.

> . . . it is contemplated that the Director will assist the President in discharging the responsibility of the President for the proper coordination of federal science and technology functions. More particularly, it is expected that he will advise and assist the President as the President may request with respect to:
>
> (1) Major policies, plans and programs of science and technology of the various agencies of the federal government, giving appropriate emphasis to the relationship of science and technology to national security and foreign policy, and measures for furthering science and technology in the nation.
> (2) Assessment of selected scientific and technical developments and programs in relation to their impact on national policies.
> (3) Review, integration, and coordination of major federal activities in science and technology, giving due consideration to the effects of such activities on non-federal resources and institutions.
> (4) Assuring that good and close relations exist with the nation's scientific and engineering communities so as to further in every appropriate way their participation in strengthening science and technology in the United States and the Free World.
> (5) Such other matters consonant with law as may be assigned by the President to the office.

To implement an effort to coordinate research, the Science Adviser has been given major additional power. He participates in decisions on budgetary matters. In effect, he is at times director of the Bureau of the Budget where scientific matters are concerned. Such a statement could be made on the basis of conversations with government people in Washington but the matter has been described fairly clearly in an address by William D. Carey, Executive Assistant Director of the Bureau of the Budget. His statement was printed in the *Congressional Record* of 30 September, 1963, from which the following is quoted:

> The Bureau of the Budget has never agreed with suggestions that it should establish within its structure a Division of Science, staffed with qualified scientists and engineers, to review R. and D. proposals. To be sure, our analysis frequently requires input of sophisticated professional judgment as to technical feasibility, state of the art, and possible alternatives to a proposed line of development, as for example in the moon program or in the missile field. In recent years, however, we have been able to obtain this kind of judgment through the Office of Science and Technology and the President's Science Advisory Committee. This year, for example, we selected the areas of atmospheric science, oceanography, water research, high energy nuclear physics, basic science, and science information for special review, and we conducted this exercise jointly with the Office of the President's Science Adviser.

It is clear from this quotation that the Bureau of the Budget is inclined to listen to the Science Adviser's counsel on scientific matters. Should some question arise as to his rightful pre-eminence, he has at his disposal a battery of leading scientists who would be stronger than any group which the Bureau of the Budget might assemble. I need not dwell on the real power that monetary control gives. Even the most powerful agency head must worry about his budget. I know of no evidence that this budgetary power has been misused but its very existence cannot but colour relations between the adviser and various agencies of the government.

Another unofficial source of power is activity in the appointments of top-level scientific personnel. When an important post becomes vacant, the Science Adviser, among others, is consulted in filling it. In view of the various positions he formally occupies he has a considerable voice in the making of such appointments.

Adviser and the Advised

It seems evident that the Science Adviser, whoever he may be, has available an ample machinery of power. The manner in which the structure functions, however, depends crucially on human factors—among them the temperament of the President, the relations between the President and his Science Adviser and the temperament of the adviser. For instance,

President Kennedy displayed considerable interest in science and the President's Science Adviser had more and easier access to Mr. Kennedy than any other scientist had. In the *New York Times* Magazine Section of 3 September, 1961, Mr. Finney has said:

> There is also a close, informal relationship between the President and the young engineer who was one of the campaign confidants and advisers. Hardly a day passes that Wiesner does not talk to the President, either in person or by telephone.

Such a relationship does not go unnoticed in Washington, where agency heads and Congressmen are sensitively attuned to nuances. Access to the President gives a man power that others respect and defer to. Close association with a President carries with it liabilities as well as assets. Presidents are usually intensely political in their outlook and they tend to react to events as they occur. In the process they urgently use whatever tools are at hand. A Science Adviser enjoying the complete confidence of a President may find himself diverted to fighting a series of minor political brush fires. He must also be responsive to any special interest, however misguided, that a President may evince in scientific or technological matters. While thus occupied, the adviser must give secondary priority to longer-term problems which may be of much greater enduring significance.

The relationship between President Johnson and his Science Adviser, Dr. Hornig, has naturally not been very close. Dr. Hornig was designated by President Kennedy and before the assassination Dr. Hornig and Vice-President Johnson had little contact. During most of 1964 the President was heading what was necessarily an interim administration. With a fresh mandate, he became free to choose his own advisers and to operate in ways that suit him best. Regardless of whom he chooses as the adviser, be it Dr. Hornig or another, a new relationship will evolve.

One factor that will shape that association will be the demands of the times. If the cold war eased further, there could be a lessening of the need of the President for an adviser. If the economy were to slow down, efforts might be made to use science and technology as a means of creating new jobs. The long-term trend is towards a larger role for science and technology in most aspects of civilisation, which is likely to lead to an increase in the importance of the role of the Science Adviser.

The mode of conduct of the Science Adviser's establishment is considerably influenced by his own personality and especially by his appetite for participating in many decisions. In the course of their activities the Science Advisers may, if they wish, make recommendations concerning a wide variety of problems.

The Adviser and his Advisers

To help him the President's Science Adviser has in addition to an operating staff the benefit of two principal advisory mechanisms. The most prestigious of these is the President's Science Advisory Committee (P.S.A.C.), of which he is chairman. This committee consists of 18 distinguished scientists who customarily meet for two days each month. In the meetings an agenda of numerous items is covered. In situations like this, a full-time chairman dominates the proceedings as far as he wishes. He can select the items that appear on the agenda. He can choose the information to be presented to the committee, and even the individuals who will perform the necessary briefings. With committees of this type it is only necessary for the chairman to be able to count on the active support of four or five of the 18 to assure effective control. Members of this committee serve for four years and are appointed by the President, who, in making appointments, naturally consults the man who is both his Science Adviser and the chairman of the committee. Even a man with moderate talents could control such a situation.

The adviser has at his disposal, if he wishes to use it, essentially the total scientific capabilities of the United States, for almost everyone will respond to a request from the executive offices of the President. The usual method of employing them involves *ad hoc* panels. As an example of how a panel might work, consider the two hypothetical questions about the National Aeronautics and Space Administration's policy with respect to scientific exploration of the moon. Should all unmanned vehicles concentrate on obtaining information relevant to the Apollo programme? Should scientific experiments be conducted that apparently have no direct relevance? To answer such hypothetical questions, a panel of scientists would be convened. The executive secretary of the group would be the man who is in charge of the space desk in the Office of Science and Technology. One or more members of the panel would be members of the President's Science Advisory Committee. Others would be distinguished scientists who had participated actively in the space programme.

There are those who might inquire as to the justification for the President's Science Adviser's participating in a policy matter so central to the activities of N.A.S.A. The answer is, of course, that lunar exploration requires money and, in turn, decisions by the Bureau of the Budget.

No official list has been published which permits complete identification of panelists, but the general composition of the panels is known, in part through the work of Miss Greenfield of *The Reporter* magazine, who spent a year studying the operations of the Science Adviser's Office. She states

that "about half the mystery guests come from private industry".[2] A substantial number of the remaining half, the academic representatives, are either at present in the Cambridge area or have in the past had some connection with it, such as service in the M.I.T. Radiation Laboratory during World War II.

Actually, the panel system has serious potential weaknesses. Brilliant men come to Washington for a few days, are given detailed briefings, and then, with a limited amount of time for consideration, form opinions on matters that may involve tremendous sums of money and even the security of the nation. Their eventual recommendations can be manipulated by controlling the nature of the briefings. Their report, when issued, is in general not a public document. Thus, if it is not convenient to implement the conclusions, the report can be pigeonholed.

Advisory groups such as P.S.A.C. and the panels can be a valuable source of ideas and information but they cannot usefully make operating decisions, nor can they implement them. Since the panel members play an advisory role and since they are stationed elsewhere, they cannot be held fully responsible. They have considerable influence while at the seat of power, but once away from it, their influence diminishes.

Numerically, the 300 or so panel members who have been called upon by the adviser represent on the order of 0·01 per cent. of the nation's scientific and engineering brains. Are they our best brains? We have no way of knowing, for the identity of their owners is kept secret. Are they used effectively, or are the advice and ideas of these men circumvented? We do not know. Could the ideas involved be sharpened by suggestions and knowledge possessed by the 99·99 per cent. who are not consulted? Again, we do not know because it has not been tried.

The Adviser and his Staff

With all his duties the Science Adviser can personally attend to only a tiny fraction of the problems and developments. Although he can get much good counsel from advisory bodies, he must depend heavily on a group of full-time associates. The principal source of assistance is a staff of professional aides totalling about 16.

An official version of what this group does was presented in a statement by a former adviser on 16 October, 1963, at a hearing of a subcommittee of the House Committee on Science and Astronautics[3]:

[2] Greenfield, M., "Science Goes to Washington", *The Reporter*, XXIX (26 September, 1963), 5, pp. 20–26. The article was reprinted in *Science*, CXLII (18 October, 1963), 3590, pp. 361–367.

[3] Statement of Dr. Jerome B. Wiesner at Hearings on Government and Science before the Subcommittee on Science, Research and Development, House Committee on Science and Astronautics, 88th Congress, First Session (16 October, 1963), pp. 58–109.

> The Office of Science and Technology, since its establishment last year, has developed into a staff mechanism to serve the Special Assistant, the P.S.A.C., and the Federal Council. Slightly more than a dozen highly trained specialists in science, engineering and administration provide assistance to these activities by identifying issues, phrasing questions, gathering information from the agencies on problems in scientific fields, working closely with the Bureau of the Budget, the Council of Economic Advisers, and other Executive Office agencies, and following up the implementation of federal programs. In addition, the Office has served as the channel by which the Executive Branch meets its duties to the Congress on matters of over-all science policy.

That the Office of Science and Technology staff has large responsibilities and authority is indicated in the phrase " following up the implementation of federal programs ". One would expect this staff to be of the highest professional calibre. I was, therefore, interested during October 1963 in checking up on the qualifications of this group of so-called " highly trained specialists ". Although a few were found to be competent, they did not in sum constitute a distinguished group of scientists. A search of *Chemical Abstracts* and *Science Abstracts* revealed that during the past 10 years few of the 16 men had written for any of the publications abstracted by these services.

Nevertheless, this group collectively has exercised perhaps almost as much power as the adviser. Some of the mechanisms by which power has been exerted have proved distressing to government agency heads. For example, the director of a great national establishment, often a man of outstanding scientific stature, might receive a telephone call. The voice at the other end would say: " This is the White House calling ". The director was faced with a dilemma. Whom did the call represent? Was the Science Adviser's Office calling to pass on an order from the President, or the President's Science Adviser, or the Chairman of the President's Science Advisory Committee, or the Director of the Office of Science and Technology, or the *de facto* Chief of the Bureau of the Budget? Or was the staff member from the Office of Science and Technology who was making the call simply throwing his weight around? In the past the last has often been the right interpretation but the agency head felt well advised not to question too closely, even though he might suspect that he was being bullied by a minor member of the staff. Not infrequently the call was a request for charts and information to be used in a briefing the next day. The director had no alternative but to turn his agency upside down to meet the deadline. If the President or the Science Adviser were personally interested in the material and he failed to deliver, he might find himself in a highly embarrassing position.

On a number of occasions I have been in touch directly and indirectly with many of the major agencies in Washington. Almost all have had numerous calls from staff members in the adviser's office. It was not always the agency head to whom the request was directed. I am told that members of the staff of the Office of Science and Technology have seen fit to communicate with subordinates within agencies, sometimes without the knowledge of the responsible head. This kind of procedure can wreak havoc with morale, for when it occurs, the underlings often assume that their chief is on the decline.

About a year ago, in talking with a man relatively high up in the Space Administration, I elicited the information that the man who has the space desk at the Office of Science and Technology was in frequent communication with various subordinates in the Space Administration and that he often concerned himself with technical matters. N.A.S.A. has attempted to obtain the advice of the scientific community and for this purpose has a number of advisory groups whose membership includes Nobel Prize winners, many members of the National Academy of Sciences and most of the leading contributors to space research, such as Professor James A. Van Allen. The total membership of these advisory subcommittees is about 70. In an effort to assess the weight their advice carries, I asked the official this question: "Would you say that an underling in the Office of Science and Technology has more weight in technical decisions affecting the Space Agency than that possessed collectively by all your consultants?" The reply was, "Yes, he does." The official in question is a conscientious person, and I would not wish to seem to disparage his character or accomplishments. But the fact is that he has not demonstrated unusual proficiency in the area in which he wields so much authority.

According to the original plan, staff members of the Office of Science and Technology were to act as subordinates, making arrangements for panels and acting as go-betweens in the transmission of information from the adviser and the President's Scientific Advisory Committee to the agencies. In practice, however, P.S.A.C. and the panel members are on hand only a small part of the time. The adviser is engaged in hundreds of activities. Someone must make interim decisions and the responsibility naturally falls to the staff of the Office of Science and Technology. Thus, in reality, though it was not planned that way, a major fraction of government research policy is made and executed by a small group of men having little proven eminence or even competence in science.

The Adviser's Jurisdiction

My impression is that many of the unsatisfactory features of the adviser's conduct of his office in the past have arisen from the attempt

to deal with too many of the wrong kinds of problems. It would be far better to do a few supremely important tasks well than to be immersed in a bottomless morass of petty detail. There are important situations which from their very nature must be handled at the top level. Matters like the nuclear test ban and first-rank military questions encompassing sophisticated science and technology probably must be dealt with at the highest level. Then there are issues such as that raised by Rachel Carson's *Silent Spring* [4]; if only for political reasons, a President would feel obliged to employ high-level advice in making a response to the furore raised by such a book.

There are other problems with which the Science Adviser should grapple and for which his office is uniquely fitted. Problems involving long-term planning are an example. In the United States remarkably little attention has been given to the future and to the consequences of contemporary activities. For instance, our research grants system is profoundly affecting higher education, probably adversely, yet the trends go almost unnoticed.

Another class of problems are those transcending the competence or jurisdiction of any existing arm of government and requiring the ultimate establishment of new mechanisms. A good example is the need that arose for a new agency to take jurisdiction over space matters. The first Science Adviser, Dr. Killian, was instrumental in the creation of the National Aeronautics and Space Administration.

The present structure of the adviser's office has at least one glaring defect, which is a consequence of the formal objectives of the Office of Science and Technology quoted earlier. Some of the relevant phrases include: " the Director [Science Adviser] will assist the President in . . . the proper coordination of federal science and technology functions. . . . he will advise and assist . . . with respect to . . . (3) Review, integration and coordination of major federal activities in science and technology . . . ".

The periodic review of major programmes can be extremely useful, provided that such examinations are conducted in depth, with elaborate care and due dignity. The best brains of the nation can be recruited for such a task. This is the kind of effort the scope of which can be delineated precisely and the proceedings can lead to greater effectiveness in relevant agencies and at times to the initiation of new procedures or even to useful legislation.

In contrast the words " integration and coordination " carry with them no end of mischief. In their present context, the words are fuzzy, even utopian, in connotation. It is literally impossible to coordinate perfectly the activities of two creative scientists—let alone thousands. Thus the Office of Science and Technology is chartered to perform a task that can be

[4] (New York: Houghton Mifflin Co., 1962; London: Hamish Hamilton, 1963.)

neither sharply delineated nor actually accomplished. Moreover, since new developments are always occurring, the unattainable goal changes constantly.

In the long pull the best brains of the nation are too astute to be trapped into such an undertaking. If "integration and coordination" are to be performed by the Office of Science and Technology the burden must rest with the limited staff of the office.

On 22 November, 1963, there appeared in *Science* an editorial written by myself which seems to have continuing validity, and which may serve as a conclusion to the foregoing reflections on the problems of organising the system of scientific advice in the United States:

> In his many roles Wiesner has been required to present at least three differing visages to the world. As the President's adviser, his appropriate function has been that of self-effacing, impartial judge, often acting under tightest security. As director of O.S.T., it has been in the public interest for him to wield authority as openly as possible. His power also implicitly has required him to be a statesman of science—a deep thinker—with long-term views of evolving patterns in science and technology and of the relations among society, science, and education.
>
> The realities of politics and power dictate that the role of President's Science Adviser and its needs should transcend all other functions. Almost inevitably the secrecy necessary to that office has been carried over into the Office of Science and Technology, which attempts to keep secret even the identity of its . . . consultants. The realities of human behavior also dictate that immediate operating decisions take priority over long-term thinking.
>
> . . . Means should be found to separate functions of the P.S.A.C.-O.S.T. complex into logical packages, with no one man asked to perform more than is humanly possible. The job of President's Science Adviser is a big one which merits full-time effort. A full-time director should lead O.S.T., and he should have a far better staff. Finally, we need a Planning Office headed by a man who can think and who can marshal the wisdom of this nation in attempting to give guidance for the future.

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U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

Escape to the Endless Frontier

How can science be related to our political purposes and to our economic and constitutional system?

Don K. Price

The United States was founded at a time when philosophers were beginning to believe in the perfectibility of mankind. Ever since Benjamin Franklin and Thomas Jefferson, Americans have been inclined to put their faith in a combination of democracy and science as a sure formula for human progress.

Today that faith burns much less bright. Since the Second World War it has seemed to many, and especially to scientists, that the faith was dimmed by the mushroom cloud of the atomic bomb. The scientists who found themselves, to their great surprise, caught up in the political troubles of the contemporary world are tempted to blame their fate on their success in discovering nuclear fission: they see their tragedy, like that of Prometheus, as the result of seizing the secrets of the gods. But it seems more realistic to remind them that their own faith in inevitable progress had been dampened before Hiroshima—during the Great Depression or even before.

The earlier creed of progress had two main articles of faith, one relating to the progress of science, the other to the progress of society. The first was that men's desire for material benefits would lead society to support the advancement of science and technology, just as the profit motive would encourage the development of the economy. The second was the corollary that the advancement of science would lead society toward desirable purposes, including political freedom.

The depression gave the general public reason to doubt these beliefs, as many scientists and philosophers had already come to do. After economists and politicians lost their confidence that the individual profit motive would automatically guarantee

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economic progress, and that technological innovation would necessarily further social welfare, it became easier for the general public to share the skepticism of scientists. The leaders of the scientific world, of course, had already come to understand that science makes progress less by the effort of inventors to find solutions for the practical problems of industry or government than by the formulation of abstract theory and the search for basic knowledge. And they had much earlier given up their faith that science was certain to further either divine purpose or political progress.

The Bush Report

At the end of the Second World War, the scientists' skepticism became a basis not for despair, but for vigorous action to guarantee the progress of science. Under the leadership of Vannevar Bush, the scientists undertook to teach the nation that basic research would not be produced automatically by the efforts of industry or government to apply science and technology to their own purposes, and that as a matter of policy the government should support basic research without regard to its application. The United States had been weak in basic science, and had had to rely on Europe for the fundamental knowledge that guided the development of the spectacular new weapons during the war. Now, knowing that "basic research is the pacemaker of technological progress," the United States must provide support from government funds for the advancement of fundamental science. This argument, presented to President Roosevelt by Dr. Bush in his famous report, *Science the Endless Frontier*, reversed the traditional policy of the United States in two ways: it persuaded universities and private research institutions that they had to ask the government for financial aid, and it persuaded the government that basic science, as well as applied research, deserved support.

But although the report abandoned the traditional faith in automatic progress with respect to science, and proposed deliberate governmental policies to encourage that progress, it did not undertake to deal with the second and more general aspect of the problem—progress in social and political affairs. The relation of science to political purposes was set aside with the assurance that the progress of science is essential to "our health, prosperity, and security as a nation," and the disclaimer that science alone would provide no panacea for social problems (*1*).

The Bush report thus dealt—as, of course, Dr. Bush was asked by the President to do—with only half of the total problem of science in its relation to politics. On that half of the problem, it taught its lesson well, and the electorate learned it thoroughly. The results can be graded, in a crude way, by looking at what Congress was persuaded science is worth to the taxpayer: we are spending more dollars today on research and development than the entire federal budget before Pearl Harbor. If the lesson was an incomplete one, no one should single out the scientists for blame. Dr. Bush was not asked by the President to revise our political philosophy, but only to present a plan for the support of science. It is curious, in retrospect, that the political questions were not raised, but the fault was not that of the scientists, but of the politicians and political scientists. There were, indeed, some arguments about such questions as how the officials should be appointed who were to make grants to scientists, and what the procedures should be for accounting and overhead payments. But these were applied details, and hardly anyone stopped to ask the fundamental question: how is science, with all its new power, to be related to our political purposes and values, and to our economic and constitutional system?

By ignoring this question, we have been trying to escape to science as an endless frontier, and to turn our backs on the more difficult problems that it has produced.

The Problems

For more than a decade, this escape seemed a sound strategy for science. Plenty of money was being provided, although there were indeed some minor inconveniences, as well as some worries in principle about the way in which basic research was subordinated to certain applied programs. But then it began to be clear, in two ways, that troubles were sure to arise in the relationship between science and politics. The first way has now become clear to everyone in practice; the second is more theoretical, and therefore more important, but less obvious.

The practical trouble has arisen because practical politicians came to doubt that the identity of purpose between government and the scientific community should be taken for granted. "Health, prosperity, and security"—it was an argument, in a more sophisticated form, that what's good for science is good for the nation. This is surely true, in a general sense, but it is no longer completely persuasive as unfriendly members of Congress begin to look for conflicts of interest between the scientific community and the nation as a whole.

Conflicts of interest appear first in petty problems, such as those of accounting for federal grants to universities. But then they appear in graver problems, like the degree to which scientists as such should have a voice in policy decisions, or government should control the direction of research and the use of its results, especially in view of the new potentialities for both good and evil of the biological and social, as well as the physical, sciences. The simple reassurance that science is bound to be good for you is not likely to be adequate. Our popular worries about intercontinental missiles and radiation fallout, in which our alarm can be directed against an alien enemy, are bad enough. But to these worries we have added the fear that scientists are about to use chemistry to poison our crops and rivers, biology to meddle with our heredity, and psychology to manipulate our ideas and our personality.

So we are about to reach the point when both scientists and politicians begin to worry not merely about specific issues, but about the theoretical status of science in our political and constitutional system, and no longer rely on the assumption—which was acceptable enough to the general public when Dr. Bush presented his memorable report—that science and democracy are natural allies. Especially since some scientists have never believed it: some have been profoundly suspicious of the American version of democratic politics, rather preferring the status of science in the more conservative and traditional societies of Western Europe, and a few have been persuaded that science would prosper better under some form of socialism.

But most scientists, of course, like most politicians, have not thought very much about the problem at all. Indeed, any reasonable foreign observer would be obliged to conclude that we

have socialized our science at best in a fit of absence of mind, and at worst with the purpose of subordinating it to the purposes of military power.

Accordingly, the scientific community and the United States generally are in even deeper trouble for their lack of a theory of the politics of science than for their failure to solve practical problems of organization or policy. The nation that was born of the first effort in history to marry scientific and political ideas—the political heir of Franklin and Jefferson—is apt to speak of the relationship of science and politics with an air of apology, while throughout Asia and Africa the missionaries of Marxism teach the developing intelligentsia that the Communist system is the only approach to politics that is firmly grounded on the scientific method.

Insights from Science Fiction

The clearest example of this contrast, as it has percolated down from the scholarly elite to the general public, may be found in science fiction. This is a form of literature unwisely neglected by students of politics. On something like the theory that if I could write a nation's songs I would be glad to let someone else write its laws, I am inclined to think that it is the space cadets of the comic strips —and their fictional counterparts back to Jules Verne or even Daedalus— who have fired our enthusiasm for the race with the Russians to the moon. That enthusiasm is certainly shared on both sides of the Iron Curtain. But with a difference, and a difference that may be more important to the future of our political system than the amount of money that we spend on space exploration.

The difference is that the Soviet space cadet, in sharp contrast to his opposite number in Western science fiction, seems to be very conscious not only that he is in a race for prestige or power with another country, but that he has discovered the key to the use of the scientific method in human affairs. This is the materialist dialectic, which is supposed not merely to let the Communist system make the best use of science in technical matters, but to give the scientific intellect a generally dominant role in the society of the future.

This notion began to appear in Soviet space fiction long before the first Sputnik. Forty years ago Alexsei Tolstoi, with some technical help from the pioneering rocket engineer Tsiolkovski, used a new propellant to put a heroic Red Army man on Mars, where he proceeded to help organize a proletarian revolution against a decadent Martian society (*2*). More recently, it has become even clearer that the Soviet conquest of space will be a means of extending to the cosmos the spread of Marxist philosophy. Thus, as one space ship rushes through the void to its first meeting with beings from another solar system, the hero reassures his colleagues that sympathetic communication will surely be possible: "Thought, no matter where it is found, will inevitably be based on mathematical and dialectical logic." (Incidentally, the hero does not rely entirely on such spiritual comfort, for he goes on to issue tranquilizers to all hands on board.) And his comrade replies with a sententious expression of confidence that they will be congenial with the beings they are about to meet, since it is inevitable that on other worlds, as on the Earth, "humanity has been able to harness the forces of Nature on a cosmic scale only after reaching the highest stage of the communist society" (*3*).

In the West, of course, the science fiction hero is a good deal less sure that science is about to bring the cosmos to a state of perfection. As Isaac Asimov has noted, most contemporary science fiction in America is not utopian, but anti-utopian (*4*). If the hero is not full of complexes from his infancy or frustrated by romantic difficulties, he is likely to be upset by the feeling that the social system in which he lives is not all it should be. The clear-eyed young hero in his space suit (like the clear-eyed cowboy or the earlier pioneers and pathfinders) is all too likely to be betrayed by selfishness or weakness in high places. Or in the more recent and more apocalyptic stories, the hero, if any, is likely to be struggling in a world that is about to be ruined, or has been ruined, by the inability of politicians to understand and control the powers released to mankind by modern science (*5*).

A generation ago, the popular utopias were mainly in the tradition of Edward Bellamy's *Looking Backward*, which in turn was still in the tradition of Francis Bacon's *New Atlantis*: the world remade to the heart's desire by the rationalism and the power of science. But today, the few scientific utopias are not calculated to inspire much hope for humanity. Even a Marxist scientist like J. D. Bernal finds some of them repulsive because "a lack of freedom consequent on perfect organization" leads to a society in which the "Utopian seems, notwithstanding his health, beauty, and affability, to partake too much of the robot and the prig" (*6*). The anti-utopian theme, on the other hand, appears in serious pronouncements by scientists as well as in science fiction; even at meetings of scientific societies, speeches are likely to be made gloomily predicting disaster from our advance in scientific knowledge, and calling for a revival of something like traditional faith (*7*).

And if the utopias have changed, so have the horror stories. A generation or two ago the traditional symbol of political oppression had not changed since before the days of Thomas Jefferson: it was the rack of the Inquisition. If you were brought up on *Westward Ho!* and Browning's dramatic monologues, to say nothing of Jefferson and Macaulay and later political historians in the liberal tradition, you were likely to believe that the main historic threat to human freedom had been averted from the English-speaking world by the defeat of the Armada, and destroyed in America by the disestablishment of the church in Virginia. About all that was necessary to perfect the possibility of human freedom (one could learn from *Huckleberry Finn* or *Elmer Gantry*) was to destroy the last vestiges of enforced conformity in our society.

But within a few decades, the popular symbol of oppression had changed completely. The techniques of torture in *Westward Ho!* had been replaced by the more scientific methods of Orwell's *1984* or Zamyatin's *We*. A society founded on technology, rather than superstition, had become the most plausible system of tyranny.

Communist Orthodoxy

The difference between the democratic and Communist camps in the popular attitude toward the political significance of science might be dismissed as the product of frivolous fiction if it did not also appear in the writings of eminent scientists. It is tempting to hope that the Soviet scientists are really dedicated only to their science, and eager to join in an international community with their West-

ern colleagues. But it is hard to write off completely the official point of view, as expressed by Academician S. I. Vavilov, that Lenin had correctly comprehended the philosophical significance of science in general, and physics in particular, when he had "pointed out that the crisis in physics could be overcome by mastering the science of dialectical materialism. This provided a sure way for physics to surmount every kind of crisis and develop further." As a result, it is supposed to be the obligation of Soviet physicists to take the dialectic as their guide not only in their approach to politics and philosophy, but also to physics itself (*8*).

In practice, all the evidence suggests that this has very little to do with the way physicists actually work in their laboratories; if they make a few rhetorical gestures in the direction of political orthodoxy in an introductory paragraph of a scientific paper, they can write as they please on scientific subjects (*9*). But Marxist dialectic is still the orthodoxy; like other authoritarian orthodoxies, it cannot stamp out skepticism and cynicism, but it can stamp out open dissent (*10*).

Szilard's Dolphins

The scientists of democratic nations, even if they are ardent anti-Communists, take no such confident view of the role of science in their political systems. Some of this pessimism comes out when leading scientists take to science fiction as a medium. Fred Hoyle, the Cambridge University astronomer, has his hero in *The Black Cloud* sum up the British political system thus (*11*): "Politicians at the top, then the military, and the real brains at the bottom. . . . We're living in a society that contains a monstrous contradiction, modern in its technology but archaic in its social organization. . . . We [scientists] do the thinking for an archaic crowd of nitwits and allow ourselves to be pushed around by 'em in the bargain." And the late Leo Szilard, University of Chicago physicist, seems to sum up his view of American politics when he has his delightful dolphins, who are surely the most engaging heroes in recent science fiction, tell why politicians fail to solve modern problems (*12*):

> Political issues were often complex, but they were rarely anywhere as deep as the scientific problems which had been solved . . . with amazing rapidity because they had been constantly exposed to discussion among scientists, and thus it appeared reasonable to expect that the solution of political problems could be greatly speeded up also if they were subjected to the *same kind* of discussion. The discussions of political problems by politicians were much less productive, because they differed in one important respect from the discussions of scientific problems by scientists: When a scientist says something, his colleagues must ask themselves only whether it is true. When a politician says something, his colleagues must first of all ask, "Why does he say it?"

The same themes come out when scientists undertake to write explicitly about the relation of science to politics. The difference that Dr. Szilard's dolphins noted between science and politics is indeed a major difference, and one that could be a starting point for a political theory. Why, indeed, do politicians, unlike scientists, have to worry about the unstated purposes of another politician, or another government? But a great many scientists do not like to follow up on the implications of that question. It is more satisfying to argue that the straightforward scientific approach of the scientist should replace the devious and prejudiced ways of politicians, and to wonder whether the scientific revolution has indeed not made obsolete the institutions of modern democracy, or at least the present way in which they are organized and managed.

Thus a federal research administrator may complain of the scientists' lack of influence by comparison with lawyers and politicians, and argue that the federal government should have a Secretary of Science to mobilize the nation's scientific resources and coordinate all its policies from a scientific point of view (*13*). Or a great German physicist and Nobel prize winner may summon his colleagues to international discussions of their difference in ideology, and to international cooperation to end the race in atomic armaments, arguing that they need to apply to politics the methods of thinking used successfully in physics—"to think out these problems, which have arisen out of our research, in our own simple realistic manner" (*14*). And one of his colleagues in those international discussions, Dr. Eugene Rabinowitch, poses the central problem directly (*15*): "The capacity of the democratic, representative systems of government to cope with the problems raised by the scientific revolution is in question."

Dr. Szilard and Dr. Rabinowitch probably represent a distinct minority of American scientists, rather than the majority who are (or wish they were) consultants to corporations and members of Rotary Clubs, and who do not trouble their heads about political theory. But the question that this minority poses about the relation of representative government to the scientific revolution cannot be brushed off lightly. For the scientific revolution has changed not only the basic sciences themselves, but their consequent ability to produce new technology; it is this ability that has led to their new financial support by government, and changed the nature of military strategy and even of the economic and political system. It is accordingly very difficult, when speaking of the social effects of science, to distinguish it from technology; even those who keep accounts on government expenditures for research and development admit that the distinction they make between basic research and applied technology is not a precise boundary.

The relationship of the scientific and technological revolution to our system of representative government is a cogent question, both in its own right and because it has been raised with such urgency not only by those who seek to strengthen the political influence of scientists, but by others who are worried about the way in which such influence may be used.

Cautionary Words

During the early 1960's, it was a rare scientific meeting that failed to discuss two pronouncements on the relation of science to politics. The first was Sir Charles Snow's vivid story about the wartime rivalry of Tizard and Lindemann as scientific advisers to the British government. That "cautionary tale" warned us that democracy was in danger from the great gulf in understanding between the Two Cultures of science and the humanities, and from any possible monopoly on scientific advice to high political authority (*16*). The second was the farewell address of President Eisenhower, warning the nation that its public policy might "become the captive of a scientific-technological elite" (*17*).

It is easy to appreciate why President Eisenhower felt as strongly as he did. His administration had started out to cut back on expenditures for research and development, but had ended

by quadrupling them. This increase was by no means for defense alone; during his eight years in office the Congress multiplied the appropriations for the National Institutes of Health more than ninefold, giving them each year more than he had recommended. Science seemed clearly to be getting out of hand. It was almost enough to make one try to apply to the budgeting process the theory of Henry Adams that science, as it becomes more abstract, increases in geometrical progression the physical power that it produces (*18*).

The President's statement was a great shock to the scientists, especially to those who had been working with the administration rather than criticizing it in the columns of the *Bulletin of the Atomic Scientists*. President Eisenhower, indeed, quickly explained that he was not talking about science in general, but only those parts allied with military and industrial power (*19*). Nevertheless, to the typical American scientist who still believed that science had helped to liberate man from ancient tyrannies, it was disconcerting to be told by a conservative president that he had become a member of a new priesthood allied with military power.

Symptoms of Friction

Yet it had begun to seem evident to a great many administrators and politicians that science had become something very close to an *establishment*, in the old and proper sense of that word: a set of institutions supported by tax funds, but largely on faith, and without direct responsibility to political control. The terms under which this support is now given to science do not seem to many politicians to fit into the traditional ideas of Jeffersonian democracy.

From the point of view of scientists and university administrators, on the other hand, the growing dependence of science on government brings a great many problems, especially the danger of increasing government control over universities. It is hard to turn money down, but more and more scientific spokesmen are beginning to worry about the conditions that come with it (*20*). From the point of view of government, the sentiment in Congress now seems to be considerably more critical of the terms on which money is provided for scientific research. Edward Gibbon summed up the cynical 18th-century attitude toward a religious establishment by remarking that all religions were "considered by the people, as equally true; by the philosopher, as equally false; and by the magistrate, as equally useful" (*21*). And now, it seems that all sciences are considered by their professors, as equally significant; by the politicians, as equally incomprehensible; and by the military, as equally expensive.

So we are beginning to observe in the Congressional attitude toward science some of the symptoms of friction between an establishment and a secular government. The symptoms showed up, for example, in Congressman L. H. Fountain's investigations of the National Institutes of Health, wherein he sought reform by uncovering abuses in the administration of the cloistered but tax-supported laboratories. And they showed up in Congressman Wright Patman's attacks on the tax-exempt foundations—institutions which by a modern kind of *mortmain* give science a range of political initiative outside the control of politics.

These attacks do not get at the main issues. They have so far been only a minor nuisance to scientific institutions, with an effect measured mainly in the time taken to fill out accounting forms. But they are a threat because they may reflect a more fundamental uneasiness in the intellectual as well as the political world. This is an uneasiness not merely about the terms of the financial relationship between government and science, but about the question whether the growing influence of science can be kept compatible with representative government. It is, in short, the same question asked by Dr. Rabinowitch—can democratic government cope with problems raised by the scientific revolution?—but from the opposite point of view.

These attitudes, as yet, may have very little to do with the way most American scientists think, either on or off duty, and practically nothing to do with the amount of money their laboratories get in government grants. They are only a small cloud on the intellectual and political horizon of the United States. But they correspond to a much greater intellectual disturbance, over the past century and a half, in Europe, where the political faith in the alliance of science and reason with free government that was characteristic of the Enlightenment gave way in the late 19th century to various forms of scholarly despair. In America, a faith in the political rationalism of the Enlightenment tended to persist in the political thinking of scientists, even after the depression shook their confidence in the inevitability of progress. Right up to the present, American scientists have shown singularly little interest in either the conservative political theorists who tell them that scientists cannot deal with basic values or solve the major human problems, or the radical theorists who tell them that science can do so if it will only join in a political system, like Marxism, that will give it real power over society (*22*).

Even the strongest critics of the government and its scientific policies—for example, many of the contributors to the *Bulletin of the Atomic Scientists*—are surprisingly traditional in their approach to the political system. They may question the capacity of our representative institutions to cope with the scientific revolution, but they tend to propose as remedies more international good will and cooperation, adequate scientific education of political leaders and the electorate, and unbiased scientific advice for members of Congress.

It is hard to quarrel with any of these ideas. But they are a little like the remedy that was most often proposed for corruption in government during the late 19th century: more good men should go into politics. That exhortation surely did some good, but probably less than the effort to adjust our political and economic institutions to the realities of the industrial revolution. That adjustment required a great many changes, by Congress and the judiciary and administrators, but it did not follow the prescriptions of any of the single-minded political prophets. It came instead from a new way of looking at the problem: we gave up thinking about politics merely in terms of the formal Constitutional system, which had been based on an analogy with Newtonian thought—a mechanistic system of checks and balances. In the latter part of the 19th century, students of politics (if they had not given up their interest in science) might have noted with interest a new analogy: as science penetrated the structure of the molecule, and identified its elements, politicians were becoming preoccupied with the elements of politics—with parties and economic classes and pressure groups—as well as its mechanistic Constitutional balances.

A New Complexity

The scientific revolution in nuclear physics and in such fields as genetics is carrying us into a third stage of complexity. That revolution seems certain to have a more radical effect on our political institutions than did the industrial revolution, for a good many reasons. Let us note three of them.

1) *The scientific revolution is moving the public and private sectors closer together.*

During the industrial revolution, the most dynamic economic interests were more or less independent of the political system. They might depend on it, as many American corporations did by relying on tariff protection, and they might try with some success to control it, but they were not incorporated into its administrative system, they did not receive support from taxation, and the main directions of their new enterprise were controlled by their owners and managers. Today, our national policy assumes that a great deal of our new enterprise is likely to follow from technological developments financed by the government and directed in response to government policy; and many of our most dynamic industries are largely or entirely dependent on doing business with the government through a subordinate relationship that has little resemblance to the traditional market economy.

2) *The scientific revolution is bringing a new order of complexity into the administration of public affairs.*

The industrial revolution brought its complexities, and relied heavily on new forms of expertise, but it did not challenge the assumption that the owner or manager, even without scientific knowledge, was able to control the policies of a business. And the same general belief was fundamental to our governmental system: the key ideas, if not the lesser details, could be understood by the legislature and debated before the public, and thus controlled by a chain of public responsibility. In one sense this was never true; in another and more fundamental sense, I think it is still true. But it is much less apparently true today than it was, and a great many more people doubt it. The great issues of life and death, many people fear, are now so technically abstruse that they must be decided in secret by the few who have the ability to understand their scientific complexities. We were already worrying about the alleged predominance of the executive over the legislature; now we worry lest even our elected executives cannot really understand what they are doing, lest they are only a façade that conceals the power of the scientists—many of whom are not even full-time officials, but have a primary loyalty to some university or corporation—who really control the decisions. If (as I believe) this is not really true, it is nevertheless true that the scientific revolution has upset our popular ideas about the way in which policies are initiated and adopted, and in which politicians can control them and be held responsible for them. We have to reconsider our basic ideas about the processes of political responsibility.

3) *The scientific revolution is upsetting our system of checks and balances.*

From a moral or ethical point of view, the industrial revolution raised problems that were relatively simple. Everyone admitted that it was possible for economic interests to control politics, but the remedy seemed to be clear: regulate business to prevent abuses, and keep selfish business interests out of the political process. This seemed clearly the basic formula for dealing with the obvious conflict of the public interest with the special interests of business. And the formula of separation of business and government was analogous in a comforting way to the formula for the separation of church and state. A church that was not dependent on government support was able to provide an independent source of moral judgment which could help to control the ethical standards of our politics and our business. As the problems began to seem a bit complex for unaided theological opinion, the universities began to provide an additional source of more scientific, but equally independent, advice to the public on the basic value judgments that should govern our policies. This was the fundamental system of checks and balances within our society: the check on practical political affairs imposed by sources of utterly independent criticism, based on a system of values that was not corrupted by the political competition for wealth or power.

But the scientific revolution seems to threaten to destroy this safeguard in two ways. First, it has gradually weakened the moral authority of religious institutions by the critical skepticism that it has made predominant in Western intellectual life, most notably in the universities. Second, it has made the universities themselves financially dependent on government, and involved them deeply in the political process. Thus, after helping to disestablish churches and free most universities from ecclesiastical control, science has now made those universities dependent on a new form of establishment, in the guise of government grants, and allied them more closely with a military power that is capable of unlimited destruction.

These three developments make some of our traditional reactions—our automatic political reflexes—unreliable in dealing with our present problems. We are automatically against socialism, but we do not know how to deal with an extension of governmental power over the economy that technically leaves ownership in private hands. It is almost an instinct with us to distrust the political bosses who, by controlling the votes of the ignorant masses, seek personal profit or power without accepting official responsibility. But we do not know how to deal with irresponsible influence that comes from status in the highest sanhedrin of science, untainted by any desire for personal profit. And we are fanatically against the public support of any institutions that might impose religious values on public policy, but when the institutions of organized skepticism tell us what science believes or how much money science needs, we have no reliable procedure for questioning their infallibility, or even for criticizing their budgets.

Reverse Twist

Science has thus given our political evolution a reverse twist. It has brought us back to a set of political problems that we thought we had disposed of forever by simple Constitutional principles. These are the problems of dealing not only with territorial subdivisions of government, and not only with economic interests and classes, but also with various groups of citizens which are separated from each other by very different types of education and ways of thinking and sets of ideals. This was the problem of the medieval estates.

The three estates of the realm, whose customary privileges grew into constitutional functions, were the clergy, the nobility, and the burgesses—those who taught, those who fought, and those who bought and sold. In our impatience with privilege at the time of the American Revolution, we abol-

ished the estates in our political system so thoroughly that we have almost forgotten what the word meant. To abolish the first estate, we disestablished the church and provided secular education through local governments. To abolish the second, we forbade titles of nobility, made the military subordinate to civil authority, and relied on a popular militia rather than a standing army. To abolish the third, we did away with property qualifications on voting and exalted freedom of contract and competition above legislative interference.

But now the results of scientific advance have been to require federal support of education and the appropriation of a tithe of the federal budget for research and development, to set up the most powerful and professional military force in history, and to make free competition a minor factor in the relationship to government of some of the major segments of the economy.

Thus we are left to face the second half of the problem which we were afraid to face during the depression, and tried to escape at the end of the Second World War: the necessity for discovering a new basis for relating our science to our political purposes. We learned half of our lesson from the scientists: the lesson that we could not have a first-rate scientific establishment if we did not understand that first-rate science depended on fundamental theoretical work and required the support of basic research for its own sake, and not merely as a by-product of applied science. Now the outlines of the second, or political half of our problem are becoming more clear. Basic science as such became steadily more powerful as it freed itself from the constraints of values and purposes. As an institution in society, it had to free itself in an analogous way from subordination to the applied purposes of the industrial corporation or the government bureau or the military service. And in the unpredictability of its progress it challenges the old notion that in matters of public policy the scientist must be controlled completely by purposes defined by politicians. So we must face the possibility that science will no longer serve as a docile instrument toward purposes that are implicit in a system of automatic economic progress, or even toward purposes that are defined for scientists by business and political leaders. In short, we can no longer take it for granted that scientists will be "on tap but not on top."

Accordingly, we need to consider not only the practical relation of scientific institutions to the economy and the government, but also the theoretical relation of science to political values, and to the principles that are the foundation of the constitutional system. Only with the help of scientists can we deal with the great issues of war and peace, of the population explosion and its effects in the underdeveloped countries, or of the dangers to our environment from our technological advances not only in weaponry but also in civilian industry and agriculture. But before we are likely, as a nation, to let science help us solve such problems, we are sure to want to know the full terms of the bargain. For although some of the political reflexes that we have acquired by several centuries of constitutional experience may be out of date, one of the most automatic is still useful: we want to know not only whether some political pronouncement is true, but why the speaker said it, having a healthy suspicion that we need to know whose interests it would further, and what its effect would be on our capacity to govern ourselves, or at least to hold our governors responsible.

The scientific community in the United States is not an organized institution, or a group with definite boundaries. It is not a hierarchical establishment. But its existence as a loosely defined estate with a special function in our constitutional system is becoming apparent, and we would do well to assess its political significance (*23*). If we do, we may find that a deeper understanding of the basic relation of science to government will help us to give it the kind of support it needs for its own purposes, as well as use it more effectively for the practical ends of public policy. And if we are willing to renounce the utopian hope that science will solve our problems for us, we may find that science by its very nature is more congenial to the development of free political institutions than our anti-utopian prophets would have us believe.

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12. L. Szilard, *The Voice of the Dolphins* (Simon & Schuster, New York, 1961), pp. 25–26.
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14. M. Born, *Physics and Politics* (Basic Books, New Yurk, 1962), pp. 66, 83.
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18. Adams predicted [*The Degradation of the Democratic Dogma* (Putnam, New York, new ed., 1958), pp. 277, 303 (first published in 1919)] that "the future of Thought, and therefore of History, lies in the hands of physicists," and went on to speculate that a rapid acceleration of thuught in the direction of the abstract sciences might "reduce the furces of the molecule, the atom, and the electron to that costless servitude to which it has reduced the old elements of earth and air, fire and water." His prediction was uncanny, except for the term "costless." For a critique of Adams' loose use of scientific metaphors in dealing with history and politics, see Joseph Mindel, "The uses of metaphor: Henry Adams and the symbols of science," *J. History Ideas*, pp. 89–102 (Jan.–Mar. 1965).
19. See the authorized interpretation by the President's Special Assistant fur Science and Technology, George B. Kistiakowsky, in *Science* 133, 355 (1961).
20. See, for example, the editorial in *Science* 140, 1365 (1963); see also the study of government-university relationships conducted by the Carnegie Fuundation fnr the Advancement of Teaching, "Twenty-six Campuses and the Federal Government," *Educational Record* (Apr. 1963), and Committee on Science and Public Policy, *Federal Support of Basic Research in Institutions of Higher Learning* (National Academy of Sciences, Washington, D.C., 1964).
21. Gibbon was ostensibly speaking of ancient Rome [*The Decline and Fall of the Roman Empire* (1788), vol. 1, chap. 2].
22. Jacques Maritain, Miguel de Unamuno, and José Ortega y Gasset represent the conservative critics of the Enlightenment: J. D. Bernal may be cited on the socialist side. Judith N. Shklar, whose *After Utopia: The Decline of Political Faith* (Princeton Univ. Press, Princeton, N.J., 1957) begins with the observation that "nothing is quite so dead today as the spirit of optimism that the very word Enlightenment evokes," goes on (p. 3) to admit that "the less reflective public, certainly until 1914, remained cheerfully indifferent to the intellectual currents of despair." In this optimistic category I would include many American scientists, and bring the date up to the present.
23. Paul M. Gross notes that the "estate" metaphor is by no means new; it was used by Arthur D. Little 40 years ago. See Gross's "The fifth estate in the seventh decade," *Science* 143, 13 (1964). Alvin M. Weinberg made a similar use of the metaphor in a commencement address at the University of Chattanooga, later reprinted in *Yale Scientific Magazine* (Oct. 1963).

SCIENCE

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U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

The Fifth Estate in the Seventh Decade

The status of science and scientists in the 1960's is reviewed.

Paul M. Gross

My distinguished predecessors of the past few years who spoke on similar occasions as retiring president of the AAAS dealt with various substantive aspects of science. At Chicago in 1959 Paul Klopsteg, in a talk reminiscent of Bragg's famous essay on the contribution of British craftsmen to British science, depicted the role played by instrumentation in the development of science. The next year, in New York, Chauncey Leake described the development of a special area of science—that of pharmacology and physiology. Last year in Philadelphia Tom Park gave us an account of the origins, development, and outcome of his own research program as an investigator in biology.

Tonight, instead of talking of my own field of physical chemistry, I think it may be of interest if I say something of the present status of science and of scientists, as I see it, from my experience of almost a half century in scientific education, research, and administration. The reference in the title to the "seventh decade" is, of course, obvious. That to the "fifth estate" may not be familiar to some. This relates to the three estates of English history—the Lords Spiritual, the Lords Temporal, and the Commons. To these was added a fourth—by Edmund Burke, according to Carlyle. Burke is said to have observed, in a famous speech: "There were Three Estates in Parliament; but, in the Reporters' Gallery yonder, there sat a *Fourth Estate* more important far than they all." If he were speaking today I am sure he would enlarge the gallery considerably and provide ample space for the commentators and columnists who, obviously, know all about the world and its affairs, both scientific and otherwise. So much for the fourth estate.

The "fifth estate" of my title can best be described in the words of the distinguished scientist and technologist Arthur D. Little, who first used this term in an address in 1924 at the centenary celebration of the founding of the Franklin Institute.

> This fifth estate is composed of those having the simplicity to wonder, the ability to question, the power to generalize and the capacity to apply. It is, in short, the company of thinkers, workers, expounders and practitioners upon which the world is absolutely dependent for the preservation and advancement of that organized knowledge which we call "science."

The status of science and scientists in the 1960's is obviously a large subject, and here I will discuss four aspects of it which I feel should claim our attention, thought, and understanding. The first relates to the greatly expanded tempo, scope, and power evident in the development of science during the past quarter of a century. Secondly, I would like to consider the increasing role of science and technology as an instrument of national policy. A third area meriting attention is a changing pattern of scientific activities and some implications of this. Lastly, and most important, for the future advancement of science, is the place of science and scientists in our modern social structure and the interactions with that structure.

Changes in Tempo, Scope, and Power

Before elaborating on these topics, I think it desirable, even at the risk of covering ground familiar to many, to sketch briefly against their historical background some of the scientific developments familiar to us. In doing this I will attempt to emphasize not the content of science so much as the changing characteristics of scientific endeavor. For this purpose it will be convenient to have two reference points in time: the late 1800's just prior to the turn of the century and the decade from 1925 to 1935.

While the ranks of the fifth estate have grown rapidly in this century, it is still true that the number of scientists remains a small fraction of the total population. In the long years prior to 1900 the voice of science in national and world affairs was rarely heard, and the individual scientist working in his ivory-tower laboratory was a little-known member of society. Nevertheless, contributions of science and technology to human welfare and to the problems of the military, the growth of industry, and economic development in general were more important with each passing decade. Toward the end of the last century and in the early years of this one a new aspect of scientific activities began to emerge. This was the concept of highly organized team activity in scientific and industrial research. It was in Germany that this concept first appeared in any substantial measure, in the latter years of the 19th

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Germany a leading position in producing such things as chemicals, pharmaceuticals, steel and machinery, and similar products of industries where scientific and technical knowledge was a prerequisite for effective production. This lead over other countries, including the United States, was retained up to World War I. Some of us can recall hearing the news in the early years of that war, before our entry, that the German submarine *Deutschland* had successfully eluded the British naval blockade and landed in Baltimore harbor. What may not be as well known is the fact that the cargo consisted of scarce pharmaceuticals and dyestuffs which sold at high prices in this country because of our almost total dependence on Germany for such synthetic chemicals. On the return voyage the cargo was mainly tungstic oxide, as tungsten was a critical raw material in many areas of Germany's advancing technology. Only after World War I and as late as the 1920's did the industrial research concept of today begin to appear as an important component of some of our own more technically based industries.

The world nitrogen supply and the fate of nations. Before this century much scientific thinking was still limited in its scope and heavily circumscribed by the walls of the laboratory. There were of course exceptions. Though the ranks of science were small in number, they included a goodly share of giants —such men as Maxwell, Rayleigh, Herz, and Röntgen, to mention but a few. One in the field of chemistry was Sir William Crookes, president of the British Association for the Advancement of Science at its Bristol meeting in September 1898. In his presidential address, after an excellent analysis of factors bearing on world food supplies, he spoke as follows.

> The fixation of nitrogen is vital to the progress of civilized humanity. Other discoveries minister to our increased intellectual comfort, luxury, or else convenience; they serve to make life easier, to hasten the acquisition of wealth, or to save time, health or worry. The fixation of nitrogen is a question of the not too distant future. Unless we can class it among the certainties to come, the great Caucasian race will cease to be foremost in the world, and will be squeezed out of existence by the races to whom wheaten bread is not the staff of life.

That these are still matters of vital interest today is seen by recalling current discussions of the population exfall relating to the sale of surplus wheat from this and other countries to help feed the millions in the Soviet bloc.

While we all have general awareness of the important role of organized industrial research in defense and in economic development, this can be focused more sharply by looking back at the events relating to the world's nitrogen supply that occurred after 1898. Based on fundamental research in Germany and Scandinavia, in the period between 1900 and World War I a new industry developed, that of nitrogen fixation. However, this was of only limited capacity at the beginning of the war. Since nitrogen is essential not only for agriculture but also for the manufacture of explosives, as war became imminent Germany began stockpiling Chilean nitrate. The first important naval engagement of World War I was fought not in the Atlantic but in the Pacific, off the coast of South America, in an operation in which British warships captured or sank a German merchant convoy carrying Chilean sodium nitrate back to Germany. With this event there were many predictions that Germany could not last long in the war, with her very limited domestic sources of nitrogen. As with many predictions, these proved quite wrong in the outcome. During the war years, the Germans, with their by then matured capability in industrial research, were able to build the first major nitrogen fixation industry in the world. Moreover, after her recovery from war and with the rebuilding of her commerce, Germany became the world's principal producer of nitrates and supplied these to Europe and the Atlantic seaboard at prices with which Chileans could not compete. The next step in this chain of economic events was a fiscal crisis in Chilean affairs, as a substantial part of the Chilean economy had been based for years on a tax on exported nitrates. Recovery from this crisis came only when, through research sponsored by American financial interests, more efficient ways were found of mining the Chilean nitre deposits and of extracting and marketing as a valuable by-product the significant amounts of iodine that they continued.

This illustration of changing conditions in the nitrate industry is but one of many that could be cited. In today's highly technological civilization the fate of nations will depend increasingly on their store of scientific knowledge ob-

their capacity and ingenuity in applying this knowledge to produce goods and provide services of all kinds. This is the basis of a sound economy a the key to its forward progress.

Rather than continuing with an account of more recent scientific and technologic events familiar to all, I will simply point out that greatly expanded basic and applied research between World Wars I and II and after World War II led to such results as the h state of development of the airplane transportation, the whole electronics industry, the release of nuclear energy and its use for power and the propulsion of naval vessels, and, finally, the successful launching of orbiting satellites.

International Geophysical Year. More detailed review of similar developments would quickly reveal much to support the thesis that there has been a greatly expanded tempo, scope, and power in activities in science during the past quarter of a century. So far as scientists themselves are concerned, this could almost be regarded as the emergence of a kind of fourth dimension in scientific thinking. Justification for such a statement is evident in a numbe directions—for example, in the th ing, planning, and execution that went into the project known as the International Geophysical Year. This was a bold frontal attack, involving international collaboration on a grand scale, which was made in an attempt to understand more fully the physical nature of the surface of our globe through a carefully planned survey of the scientific phenomena relating to the atmosphere, the oceans, and the input of radiation of all types to our near geosphere. The information gathered was vast, and the discoveries were many. Their significance for a better understanding of such important phenomena as weather changes and climatic cycles is already apparent. As the many scientists interested in s area continue working on the large number of data that were accumulated, a much deeper knowledge of the surface of our earth can be expected.

Another example of the type of thinking I have referred to, and one still on the scale of great dimen ns, is the project currently under y which is known, for short, as the Mohole." This is an attempt, fraught with great difficulty, to penetrate the earth's crustal layers to acquire a better understanding of the nature, composi-

terior core. However, such scientific thinking and progress have not been confined to endeavors of large dimensions, even global in scale. In the past decade, work of a highly competent team of mathematicians, physicists, chemists, and biochemists at Cambridge University has led to a better comprehension of the basis of life processes, through discoveries of great significance in the field of molecular biology. The determination and unraveling of the complex molecular structure of giant molecules, such as ribonucleic acid (RNA) and deoxyribonucleic acid (DNA), have been major advances and outstanding illustrations of the effective collaborative, scientific teamwork so characteristic of much current scientific activity.

Possible modification of the climatic cycle. A final example of thinking of this sort is a proposal by Ewing and others for possible modification of the age-old climatic cycle which results in repetitive glaciation of continental land masses south of the Arctic Circle. The geologic and related evidence from prehistoric times for the existence of such a cycle of ice ages with a period of perhaps 30,000 years appears clear. Ewing's thesis, in broad terms, is that the occurrence of this cycle is related to the extent of accumulation of ice and snow on the polar ice cap within the Arctic Circle and also the ingress and exit of warmer waters from the Pacific and the Atlantic over the edges of the fairly shallow geologic basin which holds the Arctic Ocean. The conclusion of the argument, which I shall not develop fully, is that this cycle could be altered by stopping or at least modifying the flow of water through Bering Strait between the Arctic Ocean and the Gulf of Alaska in the North Pacific. This would indeed be a gargantuan project in applied science, execution of which could only have been thought possible—whether desirable is another question—with the availability of nuclear explosives. Thinking of this type would, in my judgment, have occurred but rarely in earlier periods of the development of science.

An Instrument of National Policy

Much that I have outlined is evidence of the increasing role of science and technology as an instrument of national policy—the second topic under discussion. An illustration from behind the Iron Curtain at once comes to mind. Russia is the world's leading exponent of a political ideology, though many take complacent comfort in the disparity between the present standard of living in Russia and our own. Nevertheless, Russia has forged ahead through the encouragement of science, through the systematic employment of the methodology of research and development, and through the extension to large segments of her population of free education oriented strongly toward rigorous training in science and technology. Nicholas DeWitt, who has studied the Soviet manpower and educational system intensively, states the situation in the postscript to his book *Education and Professional Employment in the U.S.S.R.*

> If the aim of education is to develop a creative intellect critical of society and its values, then Soviet higher education is an obvious failure. If its aim is to develop applied professional skills enabling the indivdual to perform specialized, functional tasks, the Soviet higher education is unquestionably a success, posing not only a temporary challenge, but a major threat in the long-run struggle between democracy and totalitarianism.

While DeWitt's first description of the aim of education may well give us pause when we think of values in relation to our own culture and society, and make us ask how well our own system of education has done, the validity of his concluding statement becomes apparent from the perspective of little more than a third of a century. In this short period Russia rose from the rank of a third-rate power to a position, today, second only to that of our own country.

National defense. There is a final element relating to the present role of science and technology as an instrument of national policy which must be mentioned. This concerns warfare and the preparation for warfare, or what is today euphemistically called national defense. From the first development of gunfire in the 14th century there had been little real innovation in the practice of warfare until this century, though the Civil War did bring the introduction of steel armor and the submarine. In World War I, gas warfare, tanks, and the aeroplane made their appearance. The development of the latter for military use between the two wars paced and enhanced the great development of commercial aviation, and this, in turn, has reduced passenger traffic on our widespread network of railroads to a fraction of its volume in the first third of the century. In World War II, radically new concepts craft, and, of course, nuclear explosives were introduced. The war also saw the refurbishment and effective use of a very old device—the rocket. This was first used as a weapon by the Mongols about the middle of the 12th century, and it reappears from time to time in the subsequent history of warfare in various military versions. Francis Scott Key witnessed one of these occasions when he was a prisoner in a ship in the British fleet off Baltimore at the siege of Fort McHenry in 1814, and the spectacle inspired the line in our national anthem: "And the rocket's red glare, the bombs bursting in air. . . ."

The high state of effectiveness to which rockets were brought toward the end of World War II through intensive research and development and the advent of the German V-I's and V-II's provided the background for today's missile technology. Further development of long-range offensive missiles provided the launch rockets for orbiting satellites and for vehicles for space exploration. These are some of the advances that have completely changed the whole aspect of warfare in less than a third of a century. In this area there can be no doubt that scientific advance and capability are indispensable instruments of national policy.

Changing Pattern of Scientific Activities

What, then, have been the effects of this great expansion of science and technology, this changed scientific thinking, this involvement with national policy, on science itself, on its organizational patterns, and on scientists and their pursuit of scientific endeavor? The question brings me to my third topic. These effects have been both major in scope and diverse in direction. They have been both favorable and unfavorable for the sound advancement of scientific endeavor.

Consider first the positive side of the coin. Today, the nature and tempo of effective research requires ample funding for men, machines, and facilities, and funds have been made available in rapidly increasing measure during the past third of a century. A glance at one area, that of nuclear and high-energy physics, will quickly reveal the scale and pattern of support. From relatively small beginnings, in such laboratories as those of Rutherford and the Curies, nuclear physics in the 1920's and 1930's moved steadily but slowly ahead. Sup-

port for the first generation of high-energy machines, the early cyclotrons, came mainly from university funding and private giving by individuals or foundations. The demonstration, in the early years of World War II, of the feasibility of the nuclear chain reaction and of its significance for the release and utilization of nuclear energy in war and peace led quickly to federal support, first through the Manhattan District Project and later through the Atomic Energy Commission. The scale of this support was not in millions but in billions, and this pace continues today. However, the magnitude of expenditures, though indicative of the scale of modern scientific activity, is never a good measure of scientific achievement.

Astronomy. Nevertheless, a brief survey of several areas of science will reveal that great substantive progress has been made in recent years. A case in point is the field of astronomy. Next to mathematics, this is the oldest of the sciences, dating from Babylonian times in the 3rd century B.C., and it has a long history of achievement before 1900. The early years of this century saw the establishment, largely through private philanthropy, of a few observatories, such as that on Mt. Wilson, with telescopes and ancillary instrumentation larger and more effective, by an order of magnitude, than anything that had gone before. These were the forerunners of the large-scale scientific facilities familiar today—the giant cyclotrons, accelerators, and piles of nuclear physics. As astronomy moved ahead through the first half of the century, its progress was relatively slow by comparison with the burgeoning development of the laboratory sciences of chemistry and physics, which received much of their steadily increasing support from private, industrial, and government sources.

It was only after the establishment, at mid-century, of the National Science Foundation to support basic research that attention was turned to more adequate support for astronomy. In the middle 1950's the establishment by NSF of the Greenbank Observatory as a National Radio Astronomy Observatory marked a turning point in the character of federal support for basic science and fundamental research. From this beginning, federal funds became available for "national" basic scientific enterprises, such as the International Geophysical Year in 1957–1958 and, later, the Kitt Peak National Observatory in Arizona (near Tucson), the Mohole project, and the Atmospheric Sciences Center in Colorado. With this type of support, scientific progress in a number of relatively neglected fields, such as astronomy and various branches of the earth sciences, notably oceanography, was greatly accelerated, and the development of pure science for its own sake became, and now is, an acknowledged instrument of U.S. national policy.

The individual scientist and organized endeavor. It is of interest to consider the effects of these changes on scientists themselves. These are manifest in a number of directions, but here I mention only two, which appear to be the most significant. The first may be described as a type of dilemma with which the individual scientist appears to be increasingly confronted. In earlier periods the role of the individual scientist stood out clearly, and while the magnitude of his contribution might occasionally be large, usually it was small, though still real and discernible. Each small contribution was a piece in a growing mosaic of knowledge of the particular field involved. As this mosaic grew from initially few pieces of data and information, and as the basis for their interpretation and correlation became dimly recognized, there was ample scope for individual initiative, and there was wide freedom of choice and of action. As progress in the field increased, the few individuals with greater insight helped shape the pattern of the whole and made it part of scientific knowledge. Much of this was a seemingly random and quite haphazard process.

To this somewhat inadequate description of science in earlier years should be added the description given by Langley in his presidential address before the AAAS meeting in Cleveland in 1888. He characterized the pursuit of scientific research as "not wholly unlike a pack of hounds, which, in the long-run perhaps catches its game, but where, nevertheless, when at fault, each individual goes his own way, by scent, not by sight, some running back and some forward; where the louder-voiced bring many to follow them, nearly as often in a wrong path as in a right one; where the entire pack even has been known to move off bodily on a false scent. . . ."

Whether or not either of these descriptions is an adequate picture of earlier scientific endeavor, it is clear that, in spite of limitations of support, facilities, and equipment, there was ample room for individual freedom of choice and for the exercise of initiative, ingenuity, and resourcefulness. Out of this situation developed what we all inherit and cherish as the great tradition of freedom in science and of communication in science, both nationally and internationally. This may be stated otherwise by saying that science is universal and knows no bounds of geography, race, creed, or nationality. Many attributes characteristic of scientific endeavor in earlier periods still hold for the sharply quickened and greatly expanded domain of today's science. Unfortunately, there are signs that as this domain grows further, as it becomes more highly organized, more programmed, and more directed toward national and other ends, and as its impact on our culture and society becomes more widespread, some of this traditional freedom will be lost. An obvious example of this trend relates to freedom of exchange of information, so essential to the progress of science. In World War II it was found necessary to impose a cloak of secrecy and classification on research in the developing fields of nuclear physics—research which led to the release and utilization of nuclear energy. All agreed that this secrecy was necessary in wartime, and it was imposed under the Manhattan District Project. In the early days of the activities of the Atomic Energy Commission these restrictions were still dominant, and it was only with the passage in 1954 of the "Atoms for Peace" modification of the original Atomic Energy Act that some of them were removed or considerably relaxed.

This is one aspect of the so-called dilemma that many see ahead as the role of science becomes more important in modern civilization. Another, perhaps more important but more subtle, aspect can best be illustrated by an example from the field of chemistry. One of the great discoveries by Rayleigh and Ramsey at the end of the last century was that of the existence of the family of rare gases, the description of their properties, and the characterization of their chemical behavior. As these gases were studied further by many investigators, it became a tenet of chemical thinking that they were unreactive and would not combine with the other elements and compounds. So strong was this belief that, as theoretical knowledge of chemical reaction and

chemical binding developed through this century, an essential element of each theory of chemical bonding was ... it should account for the supposed fact that these gases would not combine chemically with anything else. The first crack in this inviolate image came from the work of Bartlett, who demonstrated the combination of the rare gas xenon to form one of the components in a coordination compound of nplex structure surrounding a central platinum atom. As often happens in science, the initial breakthrough was followed closely by others. Soon after Bartlett's discovery became known, further research and experimentation quickly destroyed this image that had dominated thinking in chemistry for some two-thirds of a century. The experiments leading to this final event need not be described in detail, but the circumstances under which they were undertaken are relevant. While I cannot claim to know these circumstances at first hand, the account as it reached me, and as it is given here, is from a source I believe to be authoritative.

Under the system in AEC national la' ·atories which provides for research ..ipation by scientists from outside ..e laboratory staff, a young physicist from a small college came to carry on research for a time at the Argonne National Laboratory. In discussing his proposed program with those responsible for general supervision of the Laboratory, he said he would like to attempt to react xenon and fluorine at an elevated temperature. Since most physical scientists were convinced that the rare gases were unreactive, and since this reaction had already been tried in the Argonne Laboratory at ordinary temperatures, it is reasonable to assume that in the discussion that ensued doubts were raised about the wisdom of devoting the investigator's time and the resources of the Laboratory to the at- mpt. If such doubts were raised, at ..ast they did not prevail, and it was agreed that the young physicist should go ahead with the attempt. The result was a spectacular, unanticipated discovery in the field of chemistry.

When a mixture of xenon and fluorine was heated in a nickel container to ...°C and then cooled rapidly to ... temperature, a deposit of white, colorless crystals of the compound xenon tetrafluoride was found, and the long-standing belief that rare gases are inert was shown to be a myth. These events came to their culmination in the summer of 1962. On learning of this discovery, many investigators at the Argonne Laboratory and elsewhere went quickly to work and made other compounds of xenon and fluorine, as well as of certain of the other rare gases. As of the end of 1963, there is already an extensive literature relating to such compounds. In passing, and somewhat out of context, I might note that *Science*, in its "Reports" section [**138**, 136 (1962)] carried the first general news of this important discovery through a communication from the Argonne group dated 2 October 1962. Incidentally, the interval of 10 days from 2 October to 12 October, the date of the issue in which the report appeared (which carried a striking picture of the crystals of xenon tetrafluoride on the cover), probably constitutes an all-time record in the rapid communication of new scientific information through the printed word.

More in the context of the present discussion of the environment in which today's scientists work was a very timely and thoughtful editorial in the same issue (p. 75) by the editor of *Science*, Philip Abelson, entitled "The need for skepticism." The last paragraph of this is well worth quoting.

> There is a sobering lesson here, as well as an exciting prospect. For perhaps 15 years, at least a million scientists all over the world have been blind to a potential opportunity to make this important discovery. All that was required to overthrow a respectable and entrenched dogma was a few hours of effort and a germ of skepticism. Our intuition tells us that this is just one of countless opportunities in all areas of inquiry. The imaginative and original mind need not be overawed by the imposing body of present knowledge or by the complex and costly paraphernalia which today surround much of scientific activity. The great shortage in science now is not opportunity, manpower, money, or laboratory space. What is really needed is more of that healthy skepticism which generates the key idea—the liberating concept.

Of serious concern under present conditions of highly organized and programmed scientific endeavor is whether the freedom, initiative, and originality of the individual will still be able to emerge to play their important roles, so evident in the history of science in earlier periods. It is disquieting to speculate on what the ultimate outcome would have been, in the case cited, if it had been decided not to make the experiment. How long would it have been before the proper conjunction of circumstances occurred again—the individual with faith in his idea and skepticism of established dogma; a laboratory with chemists experienced in handling potentially dangerous fluorine reactions; and last but not least, a supervisory group willing to authorize the trial? Here the conjunction of events was propitious, and the outcome was a brilliant success. Unfortunately, or perhaps fortunately for scientific morale, as science progresses the number of instances in which the circumstances are not propitious is unknown. We can only hope it is small.

Specialization. A subject of much current interest is the rapidly increasing degree of specialization in science, which has paralleled science's growth and expansion in the past 35 years. Consideration of this is important, because of its implications for sound scientific education and also because of the common reaction of the lay public to highly specialized activity of any sort. Specialization in the most general sense is not new. However, when we consider the complexity of our own social structure—the profusion of implements, machines, instruments, and devices—and its specialisms of all kinds, we tend to think the latter are characteristic of, and even in a measure unique in, our society and time. A moment's reflection will indicate that such is not the case. The thoughtful citizen of the great ancient metropolis of Rome, with a population of nearly 2 million persons in the 2nd century A.D., must have been confronted with something of the same situation. The highly organized civilization of the Roman Empire must have required a high degree of specialization on the part of its citizens to provide its food supply, build its aqueducts and public works, and maintain its roads and the government of its far-flung provinces and colonies—not to mention the high state of development of literature and the fine arts. It seems clear that elaborate specialization, comparable in scope to our own, has been a characteristic of all great civilizations, especially those which were highly urbanized.

Nevertheless, it is desirable to consider briefly the nature of specialization itself, particularly that in the realm of intellectual endeavor. Here the intense concentration of an individual on a limited area of special knowledge and his attainment of expertness in his field tend to break the broad pattern of uniformity of the social structure. This is especially true in a democracy. The

resulting separation of the individual from the stream of the common affairs of man tends to make the average citizen uneasy. Shaw put this feeling succinctly, in *The Doctor's Dilemma*, when he said, "All professions are conspiracies against the laity."

Much of the extensive specialization in the sciences has some features that can be most clearly delineated by the following comparison: "A salesman is one who begins by knowing a little about everything and who goes on learning less and less about more and more until he ends up knowing practically nothing about everything." On the other hand, "A specialist is one who starts off knowing a great deal about very little and goes on learning more and more about less and less until he ends up knowing practically everything about nothing."

For our present purpose the description of the salesman can be ignored. That of the specialist will bear further scrutiny. The difficulty lies in the common lay conception that what is small or restricted in scope and dimensions is simple, and in its limits amounts to "nothing." Here is one clue, and a very significant one, not only to the common negative reaction to specialization in general but to the general public's understanding of specialization in science.

Scientists, unlike the lay public, have the privilege of appreciating the accomplishments of a truly great specialist as he reveals fascinating glimpses of things to come, when, from time to time, there is a breach in the ramparts that bar us from comprehension of nature. These ramparts are long and formidably complex, as Vannevar Bush implied in his description of science as "the endless frontier." Rarely do they succumb to attack along a broad front; when they do, it is only through the work of a genius—and geniuses are rare in the human race. If science is to move forward, it will be increasingly important that the general public acquire a better understanding and some appreciation of the true nature of scientific specialization.

As we look ahead to yet unconquered areas, we may confidently predict that soundly conceived specialization in science will continue to survive and multiply. Historically, much of the effort in science has related to inanimate things, or to relatively simpler organisms or functions. As our growing knowledge permits us to move more firmly to studies of human behavior and of its psychological, biochemical, physiological, genetic, and other bases, it is possible to envisage new coalitions between psychologists, neuroanatomists, and neurophysiologists; as they grow, these coalitions may develop as specialties, as is the case for present-day biochemistry and biophysics.

With this prospect confronting us, we shall have to consider the negative aspects of the further growth of specialization and to constantly appraise its soundness. This will be especially desirable in developing sound principles to be followed in future education in the sciences. Here the danger is that the form may be mistaken for the substance. To illustrate the problem and not invoke invidious comparison, let us imagine some future specialty that we call neurobehaviorism, for want of a better designation. On what will the validity and worth of such a field, both as a contributor to our knowledge and as a field of endeavor, depend? First, it will depend on how well those in the field are versed in fundamentals of the relevant derivative sciences, such as neuroanatomy, neurophysiology, and neurobiochemistry. Beyond this, and of great importance, it will depend on how well they understand, or can acquire understanding of, principles from the underlying basic disciplines of psychology, mathematics, physics, chemistry, biology, and physiology that are relevant and applicable to the field in question.

By this criterion, the validity and worth of an area of specialization would depend on the firmness and clearness of the pathways from the outer branch to the deep, sound roots of available scientific knowledge. Perry relates an episode that occurred at Harvard in the 1830's, about Ralph Waldo Emerson and Henry Thoreau, which has point in the present context. The then-young naturalist, who was an intimate of the Emerson household, sat quietly in a corner one day while Emerson expounded to English visitors on education at Harvard, saying, "At Harvard College they teach all branches of learning." At this point Thoreau, to the embarrassment of his patron, blurted out, "Yes, but none of the roots." Without vital and continuing sustenance from strong roots, the branches of specialism will bear meager fruit.

Science in Our Modern Social Structure

Let us now turn to the fourth to[p] of this discussion of the fifth estate in the 1960's—the impacts of these changes in science on our current culture and the response and reaction of the latter to the change. I have already mentioned many of these changes and need not review them, but two additional ones deserve attention. However, before considering these let us look at a few figures for the sake of perspective.

Scientists and technologists have been, and still are, a relatively small minority group in our total population. In 1900 they numbered perhaps 90,000, representing little more than 0.1 percent of a population of about 76 million. Federal expenditures for science in 1900, similar to the federal expenditures for research and development of today, were about $10 million, or between 0.5 and 1 percent of the annual federal budget. The corresponding rough figures for 1963 are, 2.7 million scientists in a population of 190 million and federal R&D expenditures of $14 billion, which now require a[bout] 15 percent of an annual federal budget of the order of $95 billion. Thus, scientists, though their number has increased 30-fold since 1900, still comprise a relatively small part, about 1.4 percent, of the total population.

Effects of the drain on federal resources. It is important, first, to consider some of the consequences of this increasing drain on federal resources that is caused by the recent, almost exponential growth of science and technology. Since federal revenues grow at a much slower rate than the economy does as the economy advances, it is obvious that some adjustment in the growth rate of federal expenditures for science must take place. Indeed, this is already occurring, as evident to anyone who has followed recent hearings before Congress relating to the expenditures for science projected for the next annual federal budget.

One aspect of this adjustment poses a new and serious type of problem that scientists have not faced previously in any substantial measure. With l[imita]tion necessary, on what principles is the assignment of priorities to projects in the various fields of science to be made? What are the relative merits.

both in a scientific sense and from the standpoint of the national interest, of a new, large accelerator for nuclear physics, costing perhaps $100 million for its initial construction and about a third of that amount for its annual operation; of the Mohole project for drilling through the earth's crust, variously estimated to cost between $50 million and $100 million; of the expenditure of similar sums annually for biomedical research on cancer or the diseases of the heart; and of landing a man on the moon by 1970, at an estimated cost of over $5 billion? There are no clear guidelines on which to base such priority decisions, and their formulation will require a higher order of statesmanship among scientists and those in the upper echelons of government than has existed heretofore.

Effects on health. The rapid growth of science has had a second type of impact on society in our greatly expanded technological and industrial civilization. This growth has been so great that it has already begun to alter man's traditional natural environment. The emerging problems involve such things as air and water pollution, radiation hazards, occupational hazards, and contamination of milk and food supplies, which are now classed under the general head of environmental health problems.

Several years ago I was asked by the Surgeon General to head a committee of 24 members from widely diverse scientific disciplines. The group was to analyze and survey the problems in the environmental health area and to make a 10-year projection of the nation's needs for scientific research relating to environmental health and of its needs for trained manpower to deal with the problems.

These problems are varied, complex, and serious. They range from the provision of adequate sewage disposal for large and growing metropolitan districts to the recently noted higher level of radioactive contamination of caribou meat, which is an important part of the diet of Eskimos in northern Alaska.

The origin of the high levels of radioactivity in caribou meat was relatively simple to trace and understand, though not necessarily easy to control. Certain lichens on which caribou feed were found to absorb relatively larger amounts than most plants of the radioactive trace elements which the soil had received from the debris of fallout.

A simple illustration relating to the matter of sewage disposal in large metropolitan areas will show the complexity of many of the problems involved in environmental health. A number of years ago, in order to handle its sewage disposal without contaminating Lake Michigan, Chicago built a drainage canal in which water from the lake ran across country to empty into the Mississippi. The sewage effluent from Chicago was fed to this artificial running stream. The dilution of the effluent by water from Lake Michigan reduced its concentration to the point where the organic sewage could be oxidized effectively by the dissolved oxygen in the canal waters, and well-purified water was delivered to the Mississippi. As the city expanded industrially, steam plants were built along the canal, and these discharged warm water from their condensers into the stream. Ultimately, the effect of these additions of warmer water was sufficient to raise the average temperature of the canal water by some few degrees throughout the year. With this development the phenomenon technically known as "heat pollution" became operative. Simply stated, the higher temperature reduced the concentration of oxygen in the water, and therefore the capacity of the flowing stream to oxidize the organic matter present. As of several years ago this "heat pollution" had reached such proportions that its effect on the sewage disposal problem of the Chicago area was estimated to be equivalent to the effect of adding a million people to that metropolitan area.

These are but a few examples of the many effects on the economy, on health, and on various aspects of our culture and society of the greatly increased endeavors of scientists. What has been the reaction to these great changes occurring in little more than a quarter of a century? Here two things are relevant—the status of the general public's knowledge of science and its methods and, even more important, the image in the public mind of the whole modern scientific enterprise. A realistic appraisal of these two factors does not give much ground for thinking that the public has a sound comprehension of science.

The nonscientist's view of science. For the great majority of our people, formal education terminates with high school. Sober reflection about our educational system, after the launching of Sputnik, clearly revealed the woeful inadequacy of the science education of most of our people as a basis on which to build any real understanding of modern science. Since Sputnik, real improvement has been made in science teaching in many of our lower schools, but the effects of this in the adult population will not be evident for another generation.

Given this lack of any sound comprehension of science, what picture can be drawn of the image of science in the public mind? This image is difficult to describe, for it is compounded of many diverse elements. These include respect and gratitude for the "miracles" of modern medicine; admiration for the know-how of applied science which can put satellites in predetermined orbits; and awe, verging on fear, of the results of the mysterious release of nuclear energy. Two events in our time must have contributed greatly to the building of such an image, since the public, as well as most scientists, had no warning and little preparation for their advent. The first was President Truman's unheralded announcement of the dropping of the atomic bomb—a spectacular but terrible demonstration of the power of modern science. The second was the sudden news, one day in October 1957, of a second satellite orbiting our planet. This was Sputnik, the first of a growing family which later included Echo I—a "star" whose rapid course across the night sky could be easily followed with the naked eye.

What is the significance of this image, and of this lack of real understanding by the general public, for the future of science and scientists? Some already feel that scientific endeavor must be controlled and circumscribed if it results in pollution of air and water, in contamination of food with pesticide residues, in the hazards of radiation and the development of nuclear weapons. Still others, left ever farther behind in their understanding of rapid scientific advance, take refuge in a polite, but neutral, type of anti-intellectualism toward all scientific activity. The emergence of attitudes like this among nonscientists is the basis of Snow's discussion of the Two Cultures, and of his warning that the rift may grow wider unless the trend is checked.

Conclusion

Faced with these possibilities, what should we, as scientists, do? We are in some sense a privileged minority group, and all of us should be ready to exercise the grave responsibility which we all share, "to increase public understanding and appreciation of the importance and promise of the methods of science in human progress." These words are quoted from a statement of the objectives of this Association. A second objective of our organization is "to improve the effectiveness of science in the promotion of human welfare." These two should be the articles of our scientific creed in the years ahead. Furthermore, as scientists we should not lose our perspective but should recall the history of science and remember that it has survived pestilence, wars, and disaster and has surmounted barriers of race, religion, and language. Beyond this, it is even more important to recall, in a gray period of international tension, that all members of the human race, throughout its evolution and long history, have had a common opponent. This is inscrutable nature with her seemingly inexorable laws, her hosts of organisms and parasites, her hurricanes and catastrophic events of all kinds. For our human race the central problem is still that of understanding nature and attempting to control it. Here the thinking and tools of modern science have a great contribution to make. May we use them well.

Much of what I have said of warnings, of impacts and reactions, and of grave concern may have the ring of pessimism for the future as science moves swiftly ahead in one of the great adventures of the human mind. That this is not my intent can be made clear by a closing quotation from Carlyle's great satire *Sartor Resartus*. In this he attributes to his fictitious author, "Professor Teufelsdröckh Weissnichtwo," these words, in the promethean spirit of which I share: "Man's unhappiness, as I construe, comes of his Greatness: it is because there is an Infinite in him, which with all his cunning he cannot quite bury under the Finite."

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U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

Megaloscience

Because of massive organization and large budgets, scientists are heavily involved with governments.

J. B. Adams

I have chosen the title "Megaloscience" for this discussion of scientific research and its interaction with governments and universities in order to convey the impression of very large-scale scientific research with just a hint of underlying mania.

Scientists who have grown up with this activity and who are still involved in it cannot pretend to be unbiased, but we can try as objectively as possible to analyze the problems which our activities have raised and to find reasonable solutions to them. We must address our minds to these problems now, if only because governments have become very much concerned with scientific research. Partly their concern is due to the rising cost of research and partly it is due to a growing realization in political circles that scientific research and development are the mainspring of our type of civilization. This concern must ultimately lead to decisions being taken by governments, and if we are to take an effective part in the decision-making we must first clear our own minds.

Even if our thinking does no more than dispel that public image of scientific research so well summed up by Academician Artsimovich, "Scientific research is a method of satisfying private curiosity at the public expense," it will not have been in vain.

Limiting Scientific Research Budgets

To the man in the street the impressive thing about megaloscience is its apparently insatiable demand for money. Where it all goes and how it is used is a mystery to most people. What results come out are by and large incomprehensible to almost everybody, including even scientists in other fields of research.

To the astute civil servant a far more ominous characteristic is the growth rate of scientific activity. Ever since the 17th century, we are told, the number of scientists has doubled every 15 years and the cost of scientific research has doubled every 5 years. We should not, of course, accept these statements without some investigation, particularly on such points as the definition of scientist used in the statistics, but during my own professional lifetime these doubling times seem to be about right. Extrapolation of these growth rates gives the fascinating and unlikely result that all the national incomes of our countries will be spent on scientific research in the year 2000 and everybody will be scientists a few decades later. Clearly, between now and the year 2000 something must occur to limit the growth of scientific research, and our problem is to determine what the limit should be and how it can be reached without unstable oscillations.

Such figures as exist show that in countries such as the United States and

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Britain about 2½ percent of the gross national income is being spent on civil research and development. About a tenth of this, that is, ¼ percent of the total, is spent on research and the rest on development. These may appear rather small percentages compared with what is spent on seemingly trivial things such as alcoholic drink and tobacco, which between them account for nearly 10 percent, but unfortunately, if people want to spend 40 times as much on smoking and drinking as on scientific research, there seems to be very little that anybody can do to stop them. As we all know, a short life and a gay one still has its attractions, even to physicists. In my own country only about one quarter of the national income is directly spent by the government, and that goes on such items as military defense, national insurances, and other public services. If we are to determine some limit for scientific research expenditure it is probably more profitable to consider what governments do with their money than what the people at large do with theirs.

Now some of the larger countries spend as much as 10 percent of their incomes on military defense, and as a starting point it does not seem unreasonable to imagine that they could attain the same percentage for civil research and development. If the same fraction of this goes to scientific research as at present, namely one tenth, then the upper limit for scientific research would be 1 percent. Since we are currently spending ¼ percent and the doubling time is 5 years, it would only take another 10 years to reach this limit. But if we are to avoid oscillation we must approach the limit asymptotically by means of an S-shaped or logistic curve, and the exponential rise must stop at the halfway mark. In other words, we must arrest the exponential growth in 5 years' time, when the expenditures will have reached ½ percent of the national income, in order to approach the 1-percent level smoothly.

This is a very simple and perhaps naïve example, but it yields an important result, namely that if the limit is 1 percent and we want to avoid uncomfortable, if not disastrous, oscillations, we must take action in the next 5 years to stop the exponential growth of scientific research budgets. Even if the limit is 2 percent, we can only delay decisions another 5 years, and if it is less than 1 percent we must act very soon. All this is the result of the very fast growth rate of scientific budgets and it is the reason why I said earlier that we must think about these problems now.

The percentage figures I have been quoting come from published government statistics, but as we all know, there is considerable confusion in the definitions of the various forms of scientific activity, and certainly in my own country I doubt whether our present figures are a sufficiently reliable basis for action. For example, what is called scientific research, as distinct from development, is very ill-defined and differs markedly among the sciences. Also, I know of no justification for the present 1-to-10 ratio between research and development or whether this ratio should be perpetuated in the next two decades. In fact we know far too little about the whole matter, and it will take a year or so to gather reliable statistics, even given government support for national surveys. To decide on such a serious matter without these facts is surely unthinkable, at least for scientists.

Just in case my example strikes terror in the hearts of the military, I should add that the figures I have been using of 1 or even 2 percent of the national income for scientific research could easily be reached by steadily allocating, year by year, a small fraction of the normal annual increase in national incomes which most developed countries now enjoy—for example, the American gross national product is increasing at 4 percent per annum. Thus we do not need to abandon military defense in order to find money for scientific research, although if peace broke out it would be a way of absorbing military research-and-development potential into the economy.

The Organizational Scientist

Let me turn from these weighty matters and divert your attention for a moment to another remarkable aspect of megaloscience. I refer to group activity and multiple authorship of papers in scientific journals. This is particularly noticeable in the leading megaloscience of high-energy nuclear physics research, where the motto seems to be "United we publish, divided we languish." It is not only that papers have many authors but that the authors of a single paper come from many laboratories. For example, in one of the September 1964 issues of *Physical Review Letters* there are two papers, one from Brookhaven on the Ω^- hyperon with 31 authors, and the other from the European Center for Nuclear Research (CERN) on π^- meson interactions with nuclei, with 25 authors from 6 different laboratories in 5 different countries. I notice that one author of the CERN paper, the work for which was done in Geneva, explains in a footnote that his affiliation is Berkeley, California, although he is actually on leave of absence from Milan University.

Those of us who work in large laboratories know that the authors listed on a paper are by no means the only people involved in the work. The ratio of research physicists to total laboratory staff is about 1 to 7, so a piece of research with 31 authors involves on the average something like 200 people in the laboratory, and the whole effort costs the laboratory about £1 million a year. Usually what one gets for this large investment of men and money is just another small piece of a vast jigsaw. Of course one tries to plan the research so that it is a vital piece, but one cannot always be successful, and sometimes someone else puts the piece down first. Very often the vital pieces turn out to be cheaper ones and the stroke of genius which first delineates the whole pattern is usually the cheapest act of all. Nevertheless, without enough of the jigsaw pieces it is beyond even a genius to see the pattern, and so we must go on prising them out of nature, each one costing more than the last. I must emphasize that megaloscience is not different from other science in this respect; it is only that it is further along the exponential growth curve, where bits of information apparently cost more. How long we can afford to go on collecting them while waiting for a pattern to emerge is another question.

I have remarked earlier that the number of scientists has apparently been doubling every 15 years, ever since the beginning of modern science. I doubt whether the number of scientists of, say, the caliber of Newton, Einstein, Schrödinger, Rutherford, and Fermi is increasing at this rate, and if the growth of research budgets were dependent only on men of such high ability and deep insight, it is unlikely that the doubling period of 5 years in research expenditure could have been maintained in the last few decades. What seems to have happened is that

megaloscience has maintained the growth rate in recent years, first by becoming highly organized, and second by making the maximum use of whatever genius naturally arises in any decade.

In fact two distinct types of scientist have emerged in this process of scientific evolution, the Manager Scientist and the Pilgrim Scientist. Whereas the Manager Scientist spends a great deal of his time in his own laboratory, the Pilgrim Scientist is rarely to be found at home. While the Manager Scientist is responsible for large groups of people and for large laboratories and is familiar with the ways of governments and treasuries, the Pilgrim Scientist eschews all such contacts and responsibilities. Indeed, he is more in line with the popular image of a scientist, and he goes around fertilizing research in many laboratories. The Manager Scientist is mainly a postwar phenomenon, although some existed before. His job is to create the conditions in which good research can be carried out, and his reward is seeing it flourish about him.

I have used the term Pilgrim Scientist because it suggests a parallel with medieval times. The medieval pilgrim had a definite itinerary—certain holy places and religious houses to visit on his pilgrimage—and his itinerary depended on whether he was a Franciscan or Dominican or belonged to some other order. He was also the bearer of news, religious and otherwise, as we can read in Chaucer. The modern pilgrim scientist also has his shrines and religious houses to visit, depending on his branch of research. In high-energy nuclear physics, for example, the equivalents of the old religious houses are Berkeley, Brookhaven, CERN, and Dubna. It is as rare nowadays to find a scientist attaining pilgrim status in more than one research field as it was to find a medieval pilgrim belonging to more than one order. And just as it was customary for the medieval pilgrim to be fed, housed, and looked after by the monasteries, so the modern research laboratory must set aside funds to pay foreign pilgrim scientists and to send its own on tour.

Perhaps in medieval times there was a problem with pilgrims settling down in particularly attractive monasteries. Nowadays any pilgrim scientist who is captured more or less permanently in a foreign laboratory is said to be part of a national Brain Drain. Luckily, drains were far less common in medieval times, so no doubt the medieval pilgrims were mercifully saved from that simile.

Of course scientists have always traveled around. For example, right at the beginning of modern science there was the case of Tycho de Brahe, the Danish astronomer. He traveled quite extensively in Europe and at one time planned to settle in Basle, where he found the scientific community most congenial. This did not please the authorities back at home and finally Frederick II, King of Denmark, sent him a letter—it is dated 23 May 1576, a few years after New York Bay was discovered by Verrazano—which reads as follows:

> We, Frederick the Second, make known to all men, that we of our special favour and grace have conferred and granted in fee . . . to our beloved Tycho de Brahe, Otto's son . . . our land of Hveen, with all our tenants and servants who thereon live, with all rent and duty which comes from that . . . to use, hold, quit and free all the days of his life as long as he lives and likes to follow his studia mathematices.

The land of Hveen was an island of 2000 acres on which Tycho de Brahe built a castle and an observatory at Denmark's expense, and, what with the sinecures and grants, he became one of the richest men in Denmark.

You will observe that this letter contains all the ingredients to stop a Brain Drain: promise of money and staff and, above all, the personal touch in the letter of appointment—"our beloved Tycho de Brahe, Otto's son." You will not find that nowadays, not even in offers from American firms.

To Choose and How To Choose

Let me return again to money matters. The notion that there must be a limit to expenditure on scientific research naturally raises the problem of choosing among the different fields. We who are committed to the megalosciences must necessarily consider this problem very seriously indeed. Dr. Johnson once observed, "Depend on it, Sir, when a man knows he is to be hanged in a fortnight, it concentrates his mind wonderfully," and, indeed, a recent exchange of letters in *Physics Today* on this subject shows a power of concentration. All sorts of criteria for making choices have been put forward, such as scientific merit, technological merit, and social merit, as well as the degree of fundamentality of the research. National prestige is also clearly playing an important role in this matter, and so is international competition. We may yet find ourselves involved in the Pythagorean Games, as our athletic friends are now engaged in the Olympic Games. After all, the cost of the Tokyo Olympic Games is about the same as the cost of a 300-Gev accelerator laboratory for nuclear physics, and we already have our Gold Medals.

But before we get too involved in this matter, I think it is essential to be clear as to the motivations of scientific research. To my mind there are two basic motivations; one is the desire to do something and the other the desire to know something. The first is the motivation of applied research and development, and the second is the motivation of basic research. Because the motivations are different, the criteria for choice in these two types of scientific activity are different and should not be confused. To illustrate my point I can take an example from my own subject of plasma physics and fusion research. The motivation of the work of the Culham Laboratory is to see whether or not a controlled thermonuclear reactor can be built. In pursuing this aim we will of course learn a great deal about the plasma state of matter—in fact we must, if we are to make progress—but this is not the motivation of the work and it is not the reason why the British Government is spending £4 million a year on the Culham Laboratory. Such scientific activities as these must be judged on the basis of how successful they are in reaching their goals, and choices among them must be made on the basis of the values of the different goals to the sponsors at different times. It is quite conceivable that a laboratory such as Culham could have been motivated by a desire to know about the plasma state of matter. In this case it would fall into the basic research category and would be judged on a quite different basis from and in competition with the pursuit of other knowledge, such as that sought through research in high-energy nuclear physics or molecular biology.

Because the motivations of applied research and development are different

from those of basic research, the two activities are not directly comparable, and lumping them both together in a single research-and-development budget has caused a great deal of confusion, particularly at the government level. In practice it is probably easier for a country to decide what it wants to do than what it wants to know. What I shall now discuss is the second of these two dilemmas, namely, how to choose between the basic scientific researches.

My starting point is simply that basic research, as I have defined it, is part of scientific education. It is the pursuit of new knowledge about nature, and the other two parts of education are the preservation of this knowledge and the handing of it on to future generations. My thesis is that the three parts must be held closely together at all times because, once the unity of education is destroyed, I fear that the whole system will slowly but surely deteriorate. For hundreds of years this unity has been preserved by our universities, but the advent of megaloscience and the creation of large basic research laboratories remote from the universities can easily disrupt it. It was to counteract this danger that the founders of CERN insisted that the research physicists using that laboratory must not be given permanent contracts, since these would encourage them to settle down at CERN and cut them off from their universities and from teaching. To this day very few research physicists have permanent contracts at CERN—just enough to guarantee the scientific management of the laboratory.

This concept of the unity of education can also give us a rough way of judging the extent to which the various basic researches should be supported by a country at any time. Suppose, for example, we first determine the number of university scientists actively engaged in the different fields of basic research and then calculate the amount of money needed per year to maintain a research scientist in each field at maximum efficiency. Obviously the cost per research scientist per annum is not the same in all research fields—it depends on the scale at which operations have to be conducted. At the megaloscience stage, for example in high-energy nuclear physics, it costs about £30,000 a year to maintain a research physicist efficiently, and it is rather a waste of money to maintain him otherwise. This figure is obtained by taking the total annual budget of a laboratory, such as CERN, and dividing it by the number of research physicists working in that laboratory. Other basic research fields not needing such large equipment cost less per scientist. The basic research budget is then composed by multiplying the cost per scientist by the number of active university scientists in each field, which gives the individual budgets for each research field, and then adding the lot together to give the total budget. At least this system of determining basic research budgets is constructive and avoids subjective judgments about the relative merit of the various research fields. Surely in trying to determine what a country should know it is safer to base the support on what its active research scientists find most challenging and worthwhile and to which they are prepared to devote their lives.

Sooner or later, of course, the total basic research budget calculated in this way will exceed the limit which I discussed earlier, and this is likely to happen first in the most developed countries. We must therefore consider what will happen in countries which have not yet reached this limit and which are making available less money for basic research than is calculated by the method I have just described.

The first reaction of an active research scientist who cannot obtain the necessary research facilities in his own country is to seek them elsewhere. Thus the first result of a financial limitation of basic research is the emigration of research scientists—a phenomenon with which we are only too familiar in Europe. A study of the pattern of scientific emigration can give clues as to what is wrong with the support for the basic researches. For example, if the emigration is confined to scientists in one field of research, it probably means an unbalance in the distribution of funds. If it covers all fields, then the total funds are probably inadequate in comparison with those provided by other countries. In my experience scientists do not emigrate for trivial reasons, and it takes several years of neglect to drive them that far. Thus the emigration figures are at best a very delayed manifestation of an unbalance.

The serious consequence of scientific emigration is not that a country cannot obtain the results of basic research, for they are all published and available to anybody. It is that fewer active scientists are available in the country to teach and inspire the next generation of scientists and the whole system of scientific education begins to run down. Hence my insistence on the importance of the unity of education. Also, since the best scientists can most easily find jobs abroad, the damage to the education system is far greater than the numbers emigrating indicate.

Clearly this emigration only continues so long as one country is further along the exponential curve of scientific expenditure than the others, and ever since the war the attractive country in this respect has been the United States. However, it is reasonable to suppose that that country will reach the limit of expenditure on scientific research first and so give the other countries the opportunity to catch up. In other words, scientific emigration need be only transitory if countries recognize its causes and try to reach the common limit as soon as possible. Nevertheless, in the megalosciences the absolute size of a country, and therefore the size of its investment in basic research, becomes important. For example, in high-energy physics the sheer size and cost of modern multi-Gev particle accelerators make it impossible for a small country to build them alone, however advanced that country may be in its support for science on a percentage basis. The solution in these cases is for a number of countries to combine together in a joint project, as was done in the case of CERN for high-energy physics. The advantage of CERN, quite apart from its contributions to physics, is that European high-energy nuclear physicists no longer have to emigrate to America in order to continue their research and hence they tend to remain in Europe as a vital part of its scientific education. Ultimately, as the cost of individual pieces of equipment in the megalosciences mounts, even the largest countries or groups of countries will be driven to unite if the research is to continue, and this is already being discussed for the 1000-Gev stage in high-energy physics.

It might be thought, and I have seen it proposed, that the smaller countries should use their limited resources for applied research and de-

velopment and give up basic research, particularly at the megaloscience level. I think this notion is as dangerous as it is tempting to such countries. The active and original minds in science in these countries will not be satisfied with technology and applied research and will simply emigrate, thus reducing the standards of scientific education to a level where even the quality of applied scientists and development engineers may become inadequate for their tasks.

I have also heard the allocation of funds for basic research described as "dividing up the national cake." As I have tried to show, the method of determining research budgets should be additive, not divisive. Research budgets should be built up from the ingredients of research, which are the active scientists, and their proportions should be determined from what such people find most challenging and worthwhile in research. Also, basic research is not cake—it is bread and the staff of life of our type of civilization. We must get away from the idea that basic research is only a cultural activity or that its value to the community is some kind of "fallout" in technology and industrial processes. Of course our modern technology is a direct result of past basic research. The electronics industry is a result of J. J. Thompson's discovery of the electron, and the nuclear energy industry is a result of Rutherford's discovery of the nucleus. The cost of all the basic research that has ever been done is barely equal to the current year's increase in the gross national product of the larger countries, and without all that research it is doubtful whether they would now be enjoying any increases in prosperity. Nevertheless it is difficult to use such arguments for planning research expenditure in the future, however completely they justify research in the past. The true place of basic research is as a part of scientific education, and no industrial country these days can afford scientific illiteracy, whether it be in its universities, its industries, its government, or its people. We must therefore seek our guidance from this latter connection and merely accept the former as the natural consequence of enlightenment.

In Conclusion

Let me now try to draw together the threads of my discussion into some simple statements. I believe, for the reasons I have given, that we must be within a few years of the end of the exponential growth in scientific research which started in the time of Kepler, Galileo, and Newton and has been going on steadily during the last 400 years. Up till now this growth has been free and similar to the increase in populations which are not severely limited by food supplies or disease. I believe that we as scientists have an important part to play in the next most difficult phase in the growth of our subject, which is to bring the exponential phase smoothly toward a limit without oscillation or discord. We must use our skills as scientists on the growth of science itself.

I have mainly discussed basic scientific research as a vital part of the whole of scientific education. Even in the applied researches with definite goals which are supported by our countries because of these goals, we must continually examine our purposes. In nuclear fusion research, for example, we must be sure that nuclear fusion reactors remain worthwhile to the community and that we are making progress toward their realization. At the moment I think they are worthwhile and that we are making considerable progress, but if the time ever comes when their value is minimal and our progress questionable, I hope we will have the courage to speak out first and not wait until other people outside science find out and take appropriate action. As you know, the reason why populations do not continue to grow exponentially is that they either become diseased or exhaust their food supplies.

Megaloscience as the last phase in the long history of the growth of modern science has certainly brought a great number of problems for our generation, but it is only fair that I should end by briefly mentioning some of its less obvious blessings. Like technological fallout, they are perhaps incidental and were certainly not foreseen, but they have their importance.

Simply because it has grown so big, megaloscience has caused countries to act together in joint enterprises which, owing to their nonpolitical nature, have enabled methods of international behavior to be worked out far more quickly than has been possible in more controversial fields. CERN is a very good example of what international cooperation in scientific research can offer to small countries like the European states. The massive organization and large budgets of the megalosciences have brought scientists into headlong involvement with governments and treasuries, and although this interaction has not always been blissful, I think everybody has benefited from it. Also, the Manager Scientists and the Pilgrim Scientists have certainly opened up new channels of communication between the nations which even in nonscientific matters have remained remarkably direct and effective.

Perhaps future generations, looking back at our struggles with the growth rates of science and the limits, may well rate these incidental achievements as highly as our research results, and in terms of human welfare they may even find them to have been of greater significance.

Some Current Problems of Government Science Policy

What should be the balance between expenditures on pure and on applied science, and who should set it?

Harold Orlans

In the fall of 1963, much concern was evident in the scientific community about the course that several congressional committees would take in their inquiries into federal research and development programs; and the concern of interested parties is always evident at the time of the President's budget message and subsequent appropriations hearings in Congress. Now that the Select Committee on Government Research has completed its work and the House Subcommittee on Science, Research, and Development has finished its round of hearings and reports, I believe it would be generally conceded that the committee members and their staffs did an excellent and constructive job. Both committees—particularly the select committee chaired by Representative Carl Elliott of Alabama—broke new ground. It is not necessary to agree with every one of their recommendations to acknowledge that, under severe time pressure, they asked trenchant questions and gathered and published fresh and insightful information about the nation's gargantuan research-and-development enterprise. However, the fact that this special congressional effort was required to bring to light current and comprehensive statistics on such matters as the geographical distribution of federal R&D funds and the amount received by leading universities and companies suggests that the executive agencies responsible for informing the public about these expenditures had not been doing their job adequately. Let us hope that in the future these agencies maintain the standards of fuller and more timely reporting which have now been set with the assistance of Congress; for we can hardly expect to have either good current policies or adequate consideration of desirable new policies without comprehensive, timely, *and public* information about existing R&D programs.

As the rate of increase of federal R&D expenditures has been declining and as the volume of expenditures in major agencies like the Department of Defense, the National Aeronautics and Space Administration, and the Atomic Energy Commission has leveled off or declined, a major issue of public policy—and of public and private conflict within many agencies and their constituencies—has been posed: how much of the pie should go to basic research? Or, to put the matter another way, how much should go for research at universities, and how much for research and development in industry?

The Doctrine of the Sparrow

The answer of academic scientists is not entirely surprising: more should go to them. With a monotony that bespeaks a unison more than an originality of thought, they and their spokesmen in Washington argue that

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there is no danger of spending too much on basic research; that all "competent" university scientists should be supported to do work of their own choice; that science is an indivisible whole and all fields merit support equally (although some fields merit support more equally than others); that, while the results of any particular basic research project are unpredictable, it is not merely probable but virtually certain that the results of *all* basic research will yield a value greater than their cost. Some of the more ardent advocates of pure science even assert that the results of *any*—or *almost* any—pure research will *certainly* be rewarding, scientifically and socially, conceding only that one cannot predict precisely where or when the reward will be found or who will receive it. This may be termed a contemporary scientific version of the doctrine of the sparrow or the falling leaf—that no harm, no matter how slight, can befall a living thing without serving a higher moral purpose. As purposelessness and futility are thus vanquished in theology, if not in life, so, in the current eschatology of research, error, triviality, and important findings whose importance is unrecognized all equally serve the higher purposes of science. Thus, Alan Waterman, former director of the National Science Foundation, has declared that "The results of such [basic] research, in competent hands, are never without value. Even when no breakthroughs appear, the total effort always brings a possible breakthrough closer"; and he has spoken of "the statistical evidence [which was not, however, further identified] that most of the body of science ultimately achieves practical utility" (*1*). The fascinating justification of heavy federal expenditures on high-energy physics recently advanced by 30 distinguished physicists also dances delicately along the line of statistical likelihood—variously appraised as certain, probable, unlikely, and "not impossible" (*2*)—that these expenditures will yield a significant practical return. I do not doubt that they will yield *some* practical return: this one expects from the work of oafs, let alone that of brilliant men. The critical question—and I wish only to submit it, not to answer it—is: Will it yield a return commensurate with its cost, or greater than the return that can be anticipated from a comparable investment in other fields of science and technology (not to mention other areas of human endeavor)?

An Inconsistency

A striking inconsistency is apparent in the logic of many analyses of federal R&D policies. At times, the relation between the amount of money the federal government spends for basic research and the amount it spends for development is stressed, as when it is said that basic research expenditures are "only" x percent of the total R&D expenditures, whereas development expenditures are nx percent; therefore, it is argued, if economies are needed, the larger rather than the smaller amount should be cut, or carefully "scrutinized" (why not scrutinize both?). However, at other times, it is stressed that basic research should not be compared with development. Thus, it is, of late, increasingly contended that the basic-research expenditures of an agency should not compete with its expenditures on the development of new technology (which should compete instead with expenditures for the procurement and maintenance of existing technology, and other operating needs).

What, then, if anything, *should* basic research expenditures compete with? The answers to this question are frustratingly vague: indeed, no really satisfactory answer has yet been given, although there has been no lack of adventitious suggestions, ranging from expenditures on gambling or tobacco to some arbitrary percentage of the gross national product—all of which are proposed on the condition that they allow adequate scope for expansion. However, one significant suggestion was offered recently by the President's science adviser, Donald Hornig, in a letter to Senator Pastore, chairman of the Joint Committee on Atomic Energy, in which he stated that "the level and character of support for high energy physics must be determined and periodically reassessed in the context of . . . the overall national science program (rather than in relation to the applied research and development programs of the AEC) . . ." (*3*).

To my mind, there is still a good deal of usefulness in comparing expenditures on basic research with those on applied research and development, if only because these sums draw to one type of activity or the other men of comparable training—and I mean by this not only Ph.D.'s and Nobel prize-winners but the more numerous serfs of scientific and technical fiefdoms with mere master's and bachelor's degrees, and their auxiliary corps of glassblowers, machinists, secretaries, accountants, and groundkeepers. Although it was undoubtedly justifiable, immediately after the war, to complain that government expenditures on basic research were but a small percentage—evidently no more, and probably less, than 6 percent—of the $1 billion then spent on R&D, the 11 percent devoted to basic research in 1964 and the more than 14 percent proposed for 1966 out of some $14.6 billion (excluding capital plant and facilities) seem, on the face of it, not utterly, irredeemably, and tragically inadequate. Indeed, if the proportion of government funds going into university research were to be slightly reduced while that going into various forms of more direct aid to higher education were to be correspondingly increased, diverting a number of professors from laboratories to lecture rooms for another hour or two a week, the average quality of both education and research might well be enhanced.

If there is a portion of the R&D spectrum where national expenditures now appear patently inadequate to meet national and international needs, surely it is no longer the realms of pure science but those areas of obsolescent or inefficient civilian technology, at home and abroad, in which the prospect of private profit has been too dim to elicit enough private capital to ensure technical progress, while public expenditures have been blocked by the difficulty of devising a political formula acceptable to the major parties concerned. When the $7-million program of assistance for civilian technology proposed by the Department of Commerce a couple of years ago was rejected by Congress, it was noted in Sweden that their government was spending more—absolutely, not relatively—than the United States government on such programs. Surely, no R&D task merits greater priority today than the search for politically viable ways of utilizing engineers and scientists no longer required for military work to render industries like housing, transportation, textiles, and coal more efficient; to reduce the pollution of air, water, and soil; to improve our systems of education, medical care, and local government; and to raise the standards of living in impoverished areas of this and other nations. It is strange how much money and ingenuity are devoted to searching for indirect, accidental, and even surreptitious benefits of academic, mil-

itary, and space research and development, and how little to R&D programs of direct and evident social and economic utility. Have we become so muscle-bound politically that, like Primo Carnera, we can display our strength but not use it where it is obviously needed?

A National Budget for Basic Research?

To return to Hornig's very interesting statement that the level of support for high-energy physics "must be determined . . . in the context of . . . *the* overall national science program . . ."—and the definite article which I have italicized is not the least interesting part of this statement, since it alludes to something which simply does not exist. What is advocated here for one field of science must, in principle, be applied to any and every other clearly recognized field. It appears, in short, that the president's science adviser is advocating the preparation of a national or at least a federal budget for all fields of basic scientific research. A number of other signs point in the same direction: the greater separation of government-wide expenditures on development and on research in Special Analysis H of the 1966 federal budget; the energetic and not entirely noncompetitive efforts by committees of the National Academy of Sciences to define and project desirable budgetary levels for various fields of science; and particularly the attempt by an *ad hoc* committee appointed by Academy President Frederick Seitz to grapple with two difficult but inescapable questions about scientific allocations posed by Representative Daddario's Subcommittee on Science, Research, and Development (*4*):

> 1. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?
>
> 2. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

Although some may feel that the Academy committee has dodged its assignment, rather than confronted it squarely, by answering these questions in the form of 15 separate essays written by individual committee members, it is nonetheless gratifying to see the questions being seriously faced at last, and the resultant document (*5*) is a significant contribution to the thin but growing literature on the aggravating problem of scientific choice.

The new efforts of the National Science Foundation to examine the same problem of scientific allocations should also be noted; these were reported recently by Foundation Director Leland Haworth (*6*):

> The Foundation . . . plans to give additional emphasis to the compilation and analysis of data which bear specifically on the question of relative total levels of support and measures of apparent needs. . . . Thus, we hope eventually to be able to cite fairly precise figures relative to the average amount of total research support available to academic scientists, by field of science, and to augment such data with judgments from competent people in the various fields on the question of reasonable ranges of support levels for each discipline. . . . The problem of making interfield priority judgments should become more manageable if somewhat more complete information on a field-by-field basis can be made available.

The establishment of such a central data bank on federal research grants and contracts is to be commended and should materially assist the rational allocation of scientific expenditures by both public and private agencies—provided that the raw data are not husbanded and used by one camp or another as a weapon in its struggle for a share of limited funds, but are made freely available to all to enlarge our knowledge of national allocations to—and returns from—various fields of science. Too often in the past certain data relevant to public policies have been regarded as proprietary and released only in politically convenient tabulations. Let us, again, hope that the agencies responsible for formulating federal policies for science will adopt the same principle of the fullest possible disclosure of data that is universally accepted with regard to the data of science itself.

No one observing the Washington scene can, however, be so deluded as to believe that key decisions always are or can be made in public and based solely on considerations known to the public. The inner councils of government are always, to some extent, obscure; the passage of time gradually enlarges the public record of private deliberations while reducing both its authenticity and its relevance to future decisions; and available records of the process of decision in major scientific programs are sparse indeed. Except for such information and insight as can be gleaned from congressional hearings, evidence is not generally taken in public; deliberations proceed behind the necessary or convenient cloak of executive or legislative privilege, and the final pronouncement commonly resembles a brief for one side more than a dispassionate examination of available alternatives.

The Composition of the Jury

In this situation where verdicts are reached in private, the composition of the jury assumes a special importance: it provides, in fact, the principal visible assurance that justice is being done. The composition and method of selection of important scientific policy groups therefore merits continuing public scrutiny and discussion. Social scientists have managed to secure representation on an enlarged section of the National Research Council and an occasional appointment to the National Science Board, but none has yet been selected for the President's Science Advisory Committee. Engineers have been so dissatisfied with their status in the National Academy of Sciences that they have formed an academy of their own. The composition of the President's Science Advisory Committee was perhaps adequate to its earlier responsibilities of advising upon the value of proposed weapons systems. However, as the committee's responsibilities have broadened to the formulation of general government policies for science and technology, and as the machinery for implementing its advice has been strengthened, the committee's credentials for performing these larger tasks should be periodically reassessed. The geographic concentration of its members has fortunately been broadened by the latest round of appointments, but the addition of a few more members from industry and a few from selected fields of social science would strengthen the committee's competence to deal with some of the problems which it now faces.

Finally, a few words about what is sometimes regarded as the missing link in the establishment of national policies for science and technology: Congress. Congress has been quicker to

see, and to act upon, deficiencies in the executive's formulation and coordination of R&D policies than to remedy its own deficiencies. There is a clear need for improved mechanisms within Congress, comparable to those which have been developed in recent years within the executive, for handling the flow of scientific programs and budgets on a basis that is broadly consistent and campatible with the national interest. The appointment of a new unit in the Legislative Reference Service of the Library of Congress to provide information on scientific and technological programs and policies, the continuing work of the Daddario subcommittee, and the establishment of the new permanent Subcommittee on Research and Technical Programs of the House Committee on Government Operations, under the chairmanship of Representative Henry Reuss of Wisconsin, indicate a recognition of the problem. Is it too sanguine to foresee further Congressional steps to define national rather than sectional goals for science and technology and to enlarge the authority of Congress as a whole in the making of science policies?

Summary

The problems of government science policy I have noted are not exactly new, but each has, I believe, acquired a new degree of urgency from the pressure of events: How much should be spent on basic research and how much on civilian technology? How can reasonable allocations be made among various fields of science? Who is to make these allocations, in the executive and in Congress? The degree to which we can, by objective research and perceptive analysis, accommodate the accidents of history and politics to the changing needs of science, industry, and society will determine the degree to which we can serve not the interests of those groups and individuals (both scientists and politicians) who happen to be in positions of power, but the present needs of the nation.

References and Notes

1. A. Waterman, Director's Statement, in *Nat. Sci. Found. Ann. Rept.* **8**, xii (1958); *Science* **147**, 16 (1965). In contrast, John Jewkes argues that "the overwhelming mass of scientific thought and observation in the Western world was never designed to bring, has not brought, and is highly unlikely ever to bring the slightest improvement in material standards of living" [*Economic Journal* **70**, 14 (Mar. 1960)].
2. See R. Sachs, H. Primakoff, H. A. Bethe, and J. R. Oppenheimer, all in *The Nature of Matter: Purposes of High Energy Physics*, L. C. L. Yuan, Ed. (Brookhaven National Laboratory, Upton, N.Y., Jan. 1965); see also *Science* **147**, 1548 (1965).
3. From letter, dated 27 March 1964, from Donald Hornig to Senator John Pastore, in *High Energy Physics Program* (Joint Committee on Atomic Energy, Feb. 1965), p. 57.
4. *Basic Research and National Goals* (Committee on Science and Astronautics, House of Representatives, Mar. 1965), p. 1.
5. *Basic Research and National Goals*, report of the Committee on Science and Public Policy, National Academy of Sciences (U.S. Government Printing Office, Washington, D.C., April 1965).
6. L. Haworth, "Director's statement," in *Nat. Sci. Found. Ann. Rept.* **14**, xxv (1964).

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National Planning for Medical Research

Philip Handler

Let me say at once that I do not advocate the synoptic planning attempted by the systems analysts, or the balanced growth which is frequently taken as a prime desideratum, but, instead, recommend that those who plan a national medical research enterprise exercise skillful opportunism as they stimulate the growth of the system by relatively disjointed increments.

At first approach such planning seems simple. A small nation with limited resources of funds, facilities, and manpower need merely decide which is the most important biomedical problem in its part of the world and then direct those resources to solution of that problem. An economically well-developed nation, with substantially greater resources, might consider simply giving the entire system free rein in the expectation that its scientists will attack those problems which are important and approachable experimentally. Later, in retrospect, one might assess what had actually been accomplished. But neither approach is really acceptable. All the considerations which have been raised with respect to the allocation of some fraction of a nation's total resources to the biomedical research enterprise are equally appropriate when one attempts, in turn, to fractionate that enterprise. Accordingly, the problems posed by biomedical research in the smaller or less-developed nation *are* more simply managed than are those of the more complex nations. One cannot but feel that control of schistosomiasis or of frank malnutrition, for example, where these are endemic, is of overriding importance. Surely these much more properly command the attention of those concerned with the public health in such areas than do the more universal problems of heart disease, cancer, or genetic disorders.

For those responsible for decisions under such circumstances, I have but one counsel. Every research enterprise flourishes best when the group which is so engaged attains some meaningful, critical mass. Hence, a nation with one or two medical schools should seriously consider the possibility of developing only a limited number of research groups, each addressed to a problem of maximal concern to that nation and each large enough and so equipped and financed as to afford some prospect of success. Such success will not only have immediate relevance to the public health of the area but will effect a marked enhancement of morale and create an intellectual and political climate of richer opportunity for subsequent endeavors.

Only a handful of major clinical triumphs, such as the eradication of pellagra, penicillin therapy for syphilis, general antibiotic therapy, treatment of arthritis with steroids, and the recent accomplishments of vascular surgery, have, in the United States, served as catalysts which have opened the public purse for support of biomedical research. Those nations which, of necessity, can at present expect to mount only relatively more modest biomedical research enterprises may find it best not to engage competitively in those aspects of medical research which are under intensive investigation elsewhere. I do not mean to imply that individual scientists in smaller nations cannot successfully compete, for example, in molecular biology. Nor do I suggest that the scientists of the emerging nations must mark time for decades as they retrace from its beginnings the long evolution of medical research. Quite the contrary. The scientist born in one of the emerging nations but trained in one of the older laboratories and with access to current literature need suffer no handicap save the limitations of his own talent and of the resources which his society places at his disposal. Nevertheless, unless he can be joined by a sufficient group of competent colleagues, I believe he will best serve his own ends and those of his nation by addressing himself to a problem of unusual significance in his own locale.

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Internal and External Pressures

The 2nd NIH International Symposium on Biomedical Research has emphasized the concept that, for science generally, two significant sets of pressures determine the allocation of resources: pressures which arise from within the scientific community and those which arise from without (*1*). This concept is equally applicable to the allocation of resources within the biomedical enterprise. The pressures from without are easily identifiable. They include the general aspiration to free man of cancer, of heart disease, of infection, of malnutrition, of fears in the night; society expects, and quite rightly, that much of the total research effort shall be directly devoted to these ends. They include the expectation that the biomedical community will operate an educational system which will produce physicians in sufficient numbers to provide adequate care for all members of society. They include the expectation that those engaged in research will reproduce their kind in numbers sufficient to assure an adequate continuing supply of individuals who will pursue medical science. And it is gratifying to recognize that they include a growing expectation that man will intensify not only his exploration of the universe in which he finds himself but his exploration and understanding of himself.

The internal pressures, generated by the research community itself, are less widely experienced but, unless modified, more likely to give direction to

the conduct of research. For example, if left to its own devices, a substantial segment of the biomedical community is likely to eschew the immediate problems of disease. Some may enjoy the esthetics of enzyme kinetics, while ignoring metabolic disease; others may explore viral genetics, while ignoring the consequences of viral infection. Or, some wisp of the Zeitgeist may lead many to examine the mechanisms of carcinogenesis while none seek insights into the bases for schizophrenia. In sum, the scientific community continues to press for the vitality and expansion of the relevant scientific disciplines and for biological research at its most fundamental levels, preferring to defer direct attack upon overt disease until, in its view, the stage has been adequately set. In general, I share this approach.

It is the obligation of those charged with the responsibility for what is euphemistically called "planning for science" to be aware of both types of pressure, to admit that each is a valid criterion for decision making, and to recognize that neither set of pressures, alone, constitutes a sufficient basis for national decisions.

The Extreme Views

To be sure, each extreme view has had its exponents. At one extreme are statements such as that by Michael Polanyi (*2*), who argues, "No committee of scientists, however distinguished, could forecast the further progress of science except for the routine extension of the existing system. The pursuit of science can be organized, therefore, in no other manner than by granting complete independence to all mature scientists. The function of public authority is not to plan research but only to provide opportunities for its pursuit. To do less is to neglect the progress of science. To do more is to cultivate mediocrity and waste public money." The adherents of views such as this are numerous, and history documents their claims. Indeed, in only a handful of instances has organized society recognized a major problem and directed to it the scientists who found an appropriate solution.

In this country, for example, our Public Health Service recognized the threat posed by pellagra in our Southeast and dispatched Joseph Goldberger to investigate the problem. His triumph is now history, but it is rather ironic that, having prejudged the nature of the problem, the Public Health Service dispatched a bacteriologist to address himself to what proved to be a nutritional problem. And if this tale has any moral it is that the triumph reflected the genius of the investigator rather than the wisdom of those charged with allocating the then meager resources of the U.S. Public Health Service. How many instances of societal planning of successful major advances in the elucidation of human biology or in the understanding, prevention, or treatment of disease can one add to such a list? The development of Atabrine, understanding of the etiology of retrolental fibroplasia, the development of antiviral vaccines, and control of insect-borne diseases are among the relatively few such major, planned accomplishments. The development of new drugs by the laboratories of the pharmaceutical industry, an arm of organized society, must also be included.

On the other side of the ledger—that of the unplanned accomplishments which we owe entirely to the imagination and initiative of individual investigators—is virtually every other major advance in man's understanding of himself and of the disorders to which he is subject. Surely this history indicates that the criteria for research support which arise from within the scientific community are generally valid. In fairness, however, let it be said that large-scale public support of research and the opportunity to "plan" are recent phenomena, and this judgment must be held in abeyance.

Nevertheless, many concur with Hogben (*3*), who said, "To get the fullest opportunities for doing the kind of work which is worthwhile to themselves, scientific workers must participate in their responsibilities as citizens. Among other things, this includes refraining from the arrogant pretense that their own preferences are sufficient justification for the support which they need. This pretense, put forward as the plea that science should be encouraged for its own sake, is a survival of Platonism. Science thrives by its applications. To justify it as an end in itself is a policy of defeat."

Such statements engender much controversy—and properly so. Patently, modern society supports the laboratory of a scientist not so that he may amuse himself but, rather, in the hope that his activities will, in some measure, make possible realization of some of society's own expectations. To be sure, these expectations include, broadly, the advancement of knowledge, but this ranks well below the hope that the scientist's findings can soon be translated into some practical end. Accordingly, in this country we have attempted to manage a national enterprise which provides opportunity both for the scientific giants whose research, freely undertaken, results in "quantum jumps" in our understanding and for those scientists who seek to exploit such understanding in the common interest.

In our own time it has become apparent that planned science—here I use the term *planning* rather broadly—is feasible. There have been no planned breakthroughs, nor are there likely to be any. But there can be and there has been planned exploitation of such breakthroughs. Not even Fleming planned his astute observations, but the subsequent effort required to produce penicillin and to determine its structure was most effectively planned. Society did not plan Enders' observation of viral propagation in animal tissue in culture, but society did plan the large program which supported the development of effective antiviral vaccines. Society did not plan the observations which led to the strong suspicion that elevated concentration of serum lipid is related to the development of atherosclerosis and myocardial infarction, but society can and does plan the effort necessary to validate that conclusion and to develop means for alleviating this disorder. Watson and Crick were free scientists, engaged in a problem of their own choosing, but society could and did plan to support the broad-scale effort required to amplify their hypotheses, in the hope of bringing understanding of those phenomena which underlie genetic disorders of man, viral infectivity, and perhaps cancer.

But the administrators of science must not plan the *doing* of science. They can but plan *opportunities* for the doing of science and hope that talented, competent investigators will avail themselves of such opportunities. Effective planners may not do less and should not do more.

Planning a Research Enterprise

It becomes apparent that, in attempting to plan a national biomedical research enterprise, one must view the enterprise while simultaneously considering each of a series of seemingly independent parameters. Among these are the various diseases which ravage mankind, perhaps the organ systems of which man is built (liver, kidney, brain, and so on), the continuing vitality of each of the related scientific disciplines, and the integrity of the academic institutions in which much of the research is to be performed. One must weigh the relative importance of research done on man himself and research performed on animals or model systems; of research in the laboratory and research in the field; of research in areas clearly identifiable as "biomedical" and research, essential to an understanding of life, in tangentially related disciplines; of the support of research and the support of training for the future conduct of research; of the support of research and research training and the support of education in clinical medicine; and of hosts of seemingly lesser parameters. Each of these parameters is relevant to each decision concerning the planning and funding of individual research programs.

At this point one might visualize the development of a matrix in which each parameter has a weighted value and is brought to bear on each decision; this would be an idealized version of the approach of the operations or systems analyst. Successful development of such a matrix would seem to suffice for the total planning operation, and all one would then need to know would be the total appropriation to be made available by the state in any one year; all other decisions would then be automatic. This is an exaggerated version of what Charles V. Kidd has termed "allocation in multiple dimensions." In the exaggerated form here presented it is rather horrendous to contemplate and, no matter how conscientiously or painstakingly developed, is guaranteed to yield many decisions which time will prove to have been incorrect.

In a limited sense, however, the principle does have merit. Those encharged with planning responsibility must indeed be aware of the various criteria which are meaningful in the decision process. They must assure society that none of the meaningful parameters have been neglected, although they cannot possibly guarantee that a perfect balance among them all has been assured. Indeed, such balance is not even necessarily desirable.

Happily, in the real world, matters can proceed more easily and more successfully than the novice in planning might have thought. No nation has actually engaged in such detailed allocative planning. In most instances planning has been done, rather, in a single dimension. Resources have usually been allocated by disease or by discipline, or, in nations with university grants systems, have simply been apportioned among universities and other appropriate institutions. But for our purposes it is important to note that the other dimensions do exist, whether they are planned for or no. Each research project which is supported, or for which support has been denied, has relevance in virtually every possible planning dimension. And, in annual retrospective examination, it is imperative that the operation of the system be examined in as many dimensions as possible, so that, if necessary, corrective action may be taken. One can hope in this way to assure that certain broad priorities are operative. Probably highest among these is the assurance that, at all times, a future generation of investigators is being trained and that their number bears some reasonable relationship to the desired future magnitude of the national research enterprise. Second priority might be given the assurance that all the disciplines currently meaningful on the biomedical scene are given sufficient support to assure a vigorous national effort. Third priority might relate to the vitality of academic institutions and of individual laboratories. In fourth place might be the distribution of resources by disease categories, ranked in the order of the severity of such disorders in a given community. The fact that it is this fourth priority which is frequently given most obvious expression relates to political considerations rather than to the internal logic of the system.

It will be evident that in a nation confronted with a planning problem of this magnitude there *already* is a system in being which can be retrospectively examined and corrected. Indeed, much of what is called planning is essentially remedial in that it seeks to rectify apparent errors rather than move toward planned objectives. Planning proceeds from an existing base, and each proposed increment to the existing system can be considered rather readily from the multidimensional standpoint.

Allocations and Adjudications

These thoughts, lead, then, to consideration of the actual process whereby one establishes allocations within a budget and then adjudicates the competing claims of individuals or institutions within some category of that budget. Patently, this cannot be done in an information vacuum. The establishing of allocations is the more complex task, as it demands a weighing of the values of the internal and external pressures. These pressures certainly vary among nations, and in any one nation they must vary from time to time. In any case, they can only be designated as weak, strong, or paramount. Thereafter one requires real data descriptive of opportunities: knowledge of the number of competent investigators interested in a given area, of the physical facilities, of the number of students in training, and of the cost of doing business in a typical research group; and, most importantly, an assessment of the "state of the art" in each subfield of research endeavor—that is, an informed guess concerning when the time is right, conceptually and technologically, to increase significantly the level of effort in a given research area. Evaluation of this information and appraisal of the scientific field should permit tailoring of the demands of the scientific community to the interests of society. They yield a crude determination of the relative magnitudes of support to be given, for example, to fellowship programs, arthritis or dental research, genetics or pathology, clinical or basic research.

Such considerations are particularly germane to those components of the system which are properly called "small science"—science in which the individual professor or senior investigator and his coterie of junior colleagues are the meaningful productive and budgetary unit. Whether he works in a government-operated establishment or in a university where his work is supported by a national research grants program is inconsequential. When the funds available are less than those requested by the scientific

community (and this should always be the case, else excessive funds have been provided), competing requests can be evaluated only on the basis of intrinsic scientific merit—that is, the competence of the investigator and the imagination, soundness, and feasibility of his proposal. The evaluation can be made only by a jury of his peers, drawn from a national panel of experts. To be sure, they may share his enthusiasm for his discipline but they are not rivals, on his local scene, for prestige, salary, space, or influence. It is the lack of this evaluative process which is the cardinal weakness of a university grants system and of other purely bureaucratic administrative devices. Conversely, it is the operation of this evaluation system which is the best guarantee that society will get its money's worth.

Proposals for "big science" are rare in biomedical research. They must be examined closely both for their intrinsic value and for the harm they could do the rest of the system through imposing a drain on manpower, facilities, or funds. By and large they are foreign to the university biomedical community, and, if they are desirable at all, their operation is a proper function of government or of a contractor-agent.

The greatest advantage of incremental planning is the fact that such planning makes it possible to seize previously unforeseen opportunity. And it is here that the quasi-mathematical approach to total planning fails most seriously, since it does not take into account the manner in which science itself grows. Let us consider this in some detail.

Balanced and Unbalanced Growth

There is a great temptation for those engaged in planning to attempt to project systems of "balanced growth." Indeed, "balanced growth" has been the acknowledged objective of most of those who plan a nation's economy, its weapons systems, and its support of science generally as well as its support of biomedical research. Although planners frequently recognize that they cannot realize this ideal, this so-called balanced system is the proximate objective of their development programs. As noted by Hirschman and Lindblom (*4*), the basis for this ideal is a "faith in the existence of basic harmonies similar to the Greek belief that the truly beautiful will possess moral excellence as well." It seems opportune, therefore, to direct to your attention a recent series of papers which have taken striking exception to the concept of planning balanced growth of a large enterprise and have advocated in its stead a process which has been called "disjointed incrementalism."

Because the analogies are pertinent to the problems here considered, it seems appropriate to summarize the views of various members of the group who advocate this process. For example, Hirschman (*5*), an economist, has offered as the basic defense of *un*balanced growth the concept that an economy's resources should not be considered as rigidly fixed in amount. He argues that more resources or factors of production will come into play if development is marked by sectoral imbalances, since these will arouse private entrepreneurs or public authorities to action. In the present context, there are many analogies. For example, the existence of a large pool of investigators who lack facilities for their activities constitutes a pressure which, ultimately, will result in the construction of new and more adequate facilities. The appearance of large numbers of young men and women desirous of training in biomedical research results in pressure which leads to the development of fellowship and training programs. Recognition that a temperate bacteriophage can disappear into the genome of the host bacterium, be reproduced with that genome for many generations, and then reappear in vast numbers under adverse circumstances prompts many investigators interested in the nature of the viral origin of cancer to take a new tack in their explorations. As Hirschman has said, to the extent that the imbalance is self-correcting through a variety of mechanisms, unbalanced growth may propel the economy forward jerkily but also more quickly than by planned, balanced expansion.

Klein and Meckling (*6*), students of development policies for weapons systems, allege that a given development is both less costly and more speedy when marked by duplication, confusion, and lack of communication among people working along parallel lines. They argue against early attempts at integrating subsystems into a well-articulated, harmonious general system. They advocate, instead, the full exploitation of fruitful ideas regardless of their fit to some preconceived pattern of specifications. The principal basis for this attitude is the very fact of uncertainty. They note that the configuration to be developed is, in any case, unknown, and that knowledge increases as some of the subsystems become articulate. Knowledge about the nature of any one subsystem increases the number of clues concerning the desirable features of another, just as it is easier to fit in a piece of a jigsaw puzzle when some of the surrounding pieces are already in place. What is important is to develop the pieces; one can adjust them to each other later. This view argues for maximum support of the current enthusiasm for molecular biology even though its immediate clinical application seems remote, and for vigorous follow-up of clues to the possible viral pathogenesis of cancer even though the major psychoses remain enigmas and relatively few biologists seem to be immediately concerned with their elucidation. Similarly, it argues for full support of all the competent scientists in our midst, even though this results in overcrowding of their laboratories.

Lindblom (*7*), who has been concerned with general aspects of policy-making, takes as his point of departure a denial of the general validity of an assumption which is implicit in most of the literature on policy making—that there exists sufficient agreement to provide adequate criteria for choosing among possible alternative policies. This assumption is often questioned in contemporary social science, yet many of the most common prescriptions for rational problem-solving follow only if it is true.

Conventional descriptions of rational decision-making include the following steps: (i) clarification of the objective or values; (ii) survey of alternative means of reaching objectives; (iii) identification of consequences, including the side effects or by-products of each alternative means; and (iv) evaluation of each set of consequences in the light of the objective. However, Lindblom notes that such synoptic attempts at problem solving are not possible when, for example, clarification of objective founders on social conflict, when required information is not available or is available only at prohibitive costs, or when the problem is simply too complex for man's finite intellectual capacities. Most importantly, it does not logically follow, Lindblom argues,

that when synoptic decision-making is extremely difficult it should nevertheless be pursued as far as possible. Hence he suggests that, in many circumstances, substantial departures from comprehensive understanding are not only inevitable but desirable. I cite his thesis in detail because the analogy to me seems so close.

Working Principles

I have summarized the case for what may be called "semi-planning." What are the working principles of this approach? A few major notions are worthy of consideration. (i) An element of laissez-faire, with its attendant duplication and gaps, may well be desirable rather than abominable. (ii) Orderliness, balance, and detailed planning may be more satisfying to the planners than to the society they serve; some matters probably ought to be left to what has been called "a wise and salutary neglect." (iii) It is unwise to specify detailed objectives in advance when the means of obtaining them are virtually unknown. (iv) A rational problem-solver wants what he can get and does not try to get what he wants except after identifying what he wants by examination of what he can get. (v) Arrangements must be established whereby decision-makers are made aware of, and can react promptly to, emerging problems. (vi) Long-range planning is a valuable exercise, but long-range plans for a research enterprise which is the sum of many smaller research programs are of dubious validity.

These principles, taken in part from Hirschman and Lindblom (*4*), approximate a real world which is almost invariably characterized by unbalanced, not balanced, growth. It is the above-scale salary offered to the new appointee which is the surest guarantee of an increase in the scale. It is the existence and success of the National Science Foundation which provides the platform on which stand those who argue for establishment of a National Humanities Foundation. Instances of the principle that imbalance results in pressure for a correcting growth are commonplace. And these same principles seem entirely germane to the planning of a national biomedical endeavor which is as inherently sporadic and random as is the natural growth of science itself. Indeed, the hallmark of the competent investigator is that he seeks constantly to identify the most important problem which can be attacked with the technology currently available and limits his goals accordingly. But his attention is continually given also to developments within his own and related disciplines. He is quick to apply new information, new techniques, new apparatus. In short, he brings to research his imagination, his knowledge, and his technical know-how, and he combines these with what may best be described as a "skillful opportunism."

In my view, those responsible for the management of a national enterprise which is the sum of such individuals must do likewise. They must continually assess the major parameters of the enterprise for which they have responsibility, continuing the attack on the major public-health problems, insuring the vitality of the classic scientific disciplines and recognizing the emergence of new ones, insuring the training of new investigators and practitioners, and safeguarding the health of the medical schools and universities. The total system may then be nourished and made to grow, but by disjointed increments. For example, given a 10- or 20-percent increase in total funds, one should almost never expand support, across the board, of all existing programs by this 10 or 20 percent. Instead, one should take advantage of significant, albeit unplanned and unexpected, new knowledge of human biology or pathology, of the work of new investigators as it appears, of new approaches, new drugs, new apparatus, new facilities, new architecture, and newly awakened public interest, always utilizing the skillful opportunism characteristic of the individual investigator.

Goals may be set only in the broadest terms of ultimate objectives—for example, a general homotransplantation, effective cancer chemotherapy, a rational management of viral infections, genetic transformation as therapy for hereditary disorders or the prevention of atherosclerosis. And one can, in a general way, plan *for* the tasks ahead by providing the necessary physical plant, stimulating activity in biomedical engineering, and providing a sufficient number of specialized facilities such as animal colonies, hyperbaric chambers, and libraries. It is highly doubtful that the planner can wisely do more; he will fail in his responsibilities if he does less. And he must ever be mindful that the planning of science must be left to the working scientist.

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RESEARCH AND ECONOMIC GROWTH—WHAT SHOULD WE EXPECT?

B. R. WILLIAMS

THE use of science in agriculture, industry and medicine has made possible enormous increases in population, material standards of living, health and the expectation of life. We can expect further increases—provided that we use science for economic growth and not for nuclear destruction.

It is widely believed that the key factor in this growth is the rate of expenditure on research and development. It is also widely believed that the proportion of national output devoted to research and development is critical. Think how often it is argued in Britain that growth is held down by a failure to spend on research and development as high a percentage of national product as do the Americans. In France (and Germany and Australia) the argument tends to be that growth is held down by a failure to spend as high a percentage as the British. Recently, a leading member of the French planning commission assured me that finding the appropriate level of expenditure on research in France was really very easy: "for where expenditure is below the corresponding British level you know that it should be increased".

There is, however, no obvious logical step from the observed effects of applied science on past growth to the conclusion that *national* expenditure on *research* and *development* is the key to future national growth. Research is simply a process of adding to scientific knowledge. Sometimes new scientific knowledge has a direct influence on the technology embodied in production processes. Frequently, however, a further (and often very expensive) activity called development is required before science can affect technology. Hence the need to distinguish between science—the sum total of systematic and formulated knowledge about the real world—and technology—the sum total of formulated knowledge of the industrial arts. It is technology, and the efficient use of it, that is critical in growth. What evidence is there that in this sense the use of science is a certain derivative of current or recent research and development expenditure?

Evidence of Growth as a Function of Research and Development

In a paper on "The Economics of Research and Development"[1] Dr. J. R. Minasion set out to test the hypothesis "that productivity increases

[1] Minasion, J. R., "The Economics of Research and Development", in *The Rate and Direction of Inventive Activity*, Special Conference Series No. 13 (Princeton: National Bureau of Economic Research, 1962), pp. 93–142.

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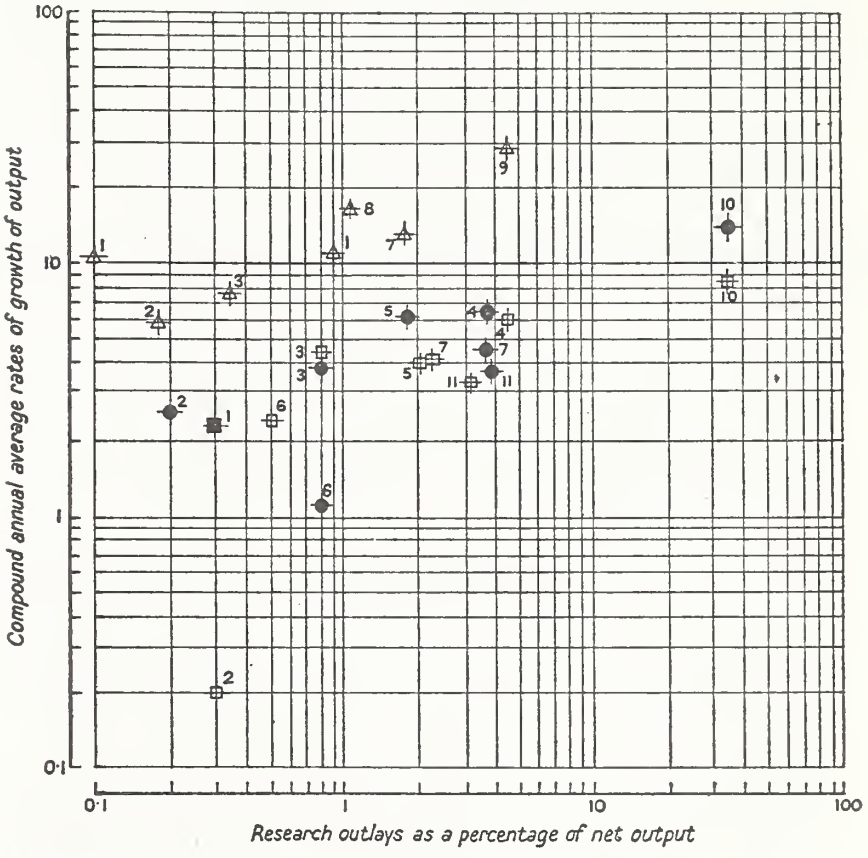
are associated with investment in the improvement of technology, and the greater the expenditures for research and development the greater the rate of growth of productivity". Minasion used a cross-section study of 18 firms in the chemical industry for the years 1947–57. He found that "the relevant research and development expenditure was a highly significant

DIAGRAM I [2]



● *United States*
△ *Hungary*
□ *United Kingdom*

Key

1 *Food*
2 *Textiles*
3 *Paper*
4 *Chemicals*
5 *Rubber*
6 *Metallurgy*
7 *Machinery*
8 *Electrical engineering*
9 *Precision engineering*
10 *Aircraft*
11 *Total industry (excluding mining, electrical energy and aircraft)*

[2] From U.N. report on *Some Factors in Economic Growth in Europe During the 1950s* (Geneva: United Nations, 1964), Chapter 5, p. 10.

independent variable explaining not only the rate of growth in productivity but also the trend of the profitability of 18 chemical firms in the sample ". He also found that lagged research and development explained end-of-period profitability better than end-of-period research, and development was explained by lagged profitability and that there was not a statistically significant relationship between the growth in productivity and the rate of change in output, average plant size, or the rate of growth in plant size. However, Minasion's sample was not necessarily representative of the chemical industry and he was unable to isolate factors such as royalty payments, which would almost certainly have influenced productivity changes. Nor, of course, could these results be legitimately generalised for the whole economy.

If we shift the measure from growth of productivity to growth of output, we do, however, find some broad but striking relationships between growth in output and research outlays as a percentage of net output. In Diagram I this relationship is plotted for 10 industry groups in the U.K., U.S.A. and Hungary for the years 1949–59.

Dr. R. A. Ewell had earlier pointed to this relationship between research and growth in U.S. industry from 1928–53.[3] He also pointed to a high correlation between the growth of national product and the country's expenditure on research and development. Assuming a causal link between research expenditure and growth he forecast that, if (on 1954 prices) research and development expenditures were increased to $6·3–6·9 thousand million by 1964, gross national product (G.N.P.) would rise to $500 thousand million. Roughly the Ewell projections were:

TABLE I

	1954	1960	1964
G.N.P. index	100	114	131
R. & D. as a percentage of G.N.P.	1·1	1·23	1·37

Ewell's confidence in these conclusions was increased by an alternative line of calculation—that between 1928 and 1953 new products created by research and development were between 11 and 22 per cent. of G.N.P. and that without them annual growth would not have been 3 per cent. but 2½–2 per cent. By imputing this growth to the median research and development expenditure for the period, he concluded that the annual yield to expenditure on research and development was 100–200 per cent. In this calculation of

[3] Ewell, R. A., " First outpost in a new frontier ", *Chemical & Engineering News*, 18 July, 1955.

yield Ewell made no allowance for outlays other than those on research and development, although on his calculations $11 of capital expenditure were required for $1 of research and development.

Implications of This and Other Evidence

Minasion's conclusions were based on a very limited sample—18 U.S. chemical firms for the years 1947–57. In Britain for the period 1949–59 no such conclusions emerged from the Freeman and Evely analysis of a sample of 44 firms in general engineering, 22 in chemicals, 12 in electrical engineering and 17 in steel. For the sample firms in chemicals and general engineering there was a positive association between growth, profitability and research ratios for the top 5 per cent. and bottom 5 per cent. of firms, but not throughout the whole range of firms. For the whole sample "additional research and development appears to make only a limited contribution to additional growth. The greater part of the differences between firms in rates of growth and profitability are due to other factors than differences in the amount of research and development done."[4]

The information in Diagram I is also rather limited. Correlations in this field are sensitive to definitions of industries. Industries as defined for statistical purposes vary greatly in their degrees of integration and it is possible for an industry with a high rate of growth in output and productivity to depend on the innovatory activities of its suppliers. Thus it matters greatly whether or not one defines the motor-car industry in Britain to include the firms which make the components.

However, the main problem here is one of interpretation. Should we conclude, or imply, that if the low growth industries had spent more on research and development they would have grown faster? The issue is not simply whether more physical output could have been produced but whether more output could have been profitably produced.

If economic growth opportunities were there but wasted by the industries with low percentage expenditures on research and development, this could mean that firms in such industries had failed to recognise (and/or exploit) opportunities for profitable investment in research and development. Or, it could mean that although the potential was there it was not possible for individual firms in the industry to realise it. This could happen where the firms in the industry were too small to finance and manage the research and development required, in which case state provision for research and development (as in agriculture), or cooperative research (as in steel, textiles, pottery, etc.), should solve the problem.

[4] *Industrial Research in Manufacturing Industry 1959–60* (London: Federation of British Industries, 1961), pp. 43–49.

There is little doubt that many firms in low-growth industries have often failed to recognise opportunities to invest in research. But whether British textile and metallurgical firms have been worse in this respect than paper firms (see Diagram I), I very much doubt. In any case one of the striking things about research and development percentages in different countries is that their industrial ranking is not very sensitive to the size of firms in the industries. This is implied in the following table which compares U.S. and British firms.

TABLE II

Research and Development as Percentage of Net Output, 1958 [5]

	U.S. Companies	U.K. Companies
Aircraft	30·9	35·1
Electronics	22·4	12·3
Other electrical	16·3	5·6
Vehicles	10·2	1·4
Instruments	9·9	6·0
Chemicals	6·9	4·5
Machinery	6·3	2·3
Rubber	2·7	2·1
Non-ferrous metals ...	2·0	2·3
Metal products	1·3	0·8
Stone, clay and glass ...	1·2	0·6
Paper	0·9	0·8
Ferrous metals	0·8	0·5
Food	0·5	0·3
Lumber and furniture ...	0·2	0·1
Textiles and apparel ...	0·2	0·3
All industries	5·7	3·1

The similarity in the pattern of industrial research and development between the U.S. and Britain is very striking. The only significant difference is in vehicles, which is partly explained by differences in industrial coverage. The main reason for the differences between industries is that the average profitability of research varies from industry to industry, depending on the state of technology and the extent of market saturation. It would clearly be quite inappropriate to conclude from the evidence of Diagram I that growth rates can be pushed up simply by raising research and development percentages.

[5] From Freeman, C., "Research and Development: A Comparison between British and American Industry", *National Institute Economic Review* (May, 1962), 20, p. 31.

An appraisal of Ewell's " evidence " helps to throw further light on this issue. Ewell's forecasts proved to be very wide of the mark. His forecast was that in real terms research and development would rise by 27 per cent. and G.N.P. by 14 per cent. In fact, research and development rose by over 150 per cent. and became 2·8 per cent. of G.N.P., which rose by only 5 per cent.[6] In 1964 research and development will be more than 3 per cent. of G.N.P., although G.N.P. will be little above Ewell's prediction for 1960. Now it may be argued that this unexpected rise in research and development expenditures after 1954 could not yet have had its full effect on growth. This may prove to be so, but the fact is that growth has not risen to the levels expected from past research expenditure. In the period up to 1954, almost one half of America's cumulative expenditure on research and development had been in the last five years. Allowing a 5–10 year time lag, the growth effects Ewell confidently forecast from research should have shown at least from 1960 on.

The basic error in Ewell's approach was that he used a bi-variate approach to a multi-variate situation. He mentioned the importance of capital expenditure, production, sales, advertising, management, etc., but implied that we can simply take them for granted. He even calculated the yields to research and development as if they were the only costs of new innovation. Ewell did not discuss the objectives of research and development, whether certain types of research are more likely to have growth potential than others, the best mixtures of research and development, the alternative uses of scientists and engineers, or the possible tendencies to diminishing returns. By now it is clear that these are important factors, and that the model implied by Ewell—which is still frequently used—is far too simple to cope with the complex relations between science and growth.

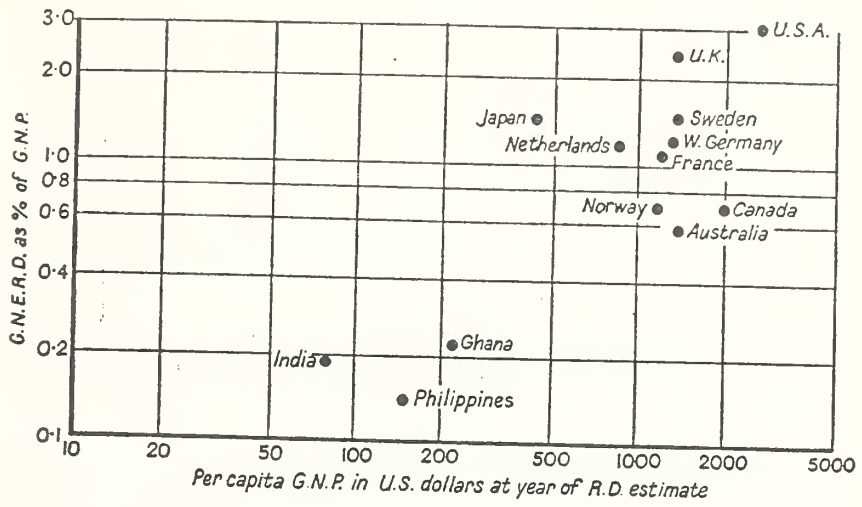
We get a further indication of the complex relations between research and growth from the relations between G.N.P. per head and the percentage expenditure on research and development in different countries. These, which are shown in Diagram II, are sometimes taken to demonstrate the tendency of the R. and D. percentage to rise with G.N.P. per head.

A tendency can be weak or strong. The tendency shown in Diagram II is very weak. Japan is obviously well " off trend ". So too are Canada and Australia. Indeed, if a vertical line is run through the West German per capita G.N.P. it is plain that for six countries with similar levels of G.N.P. per head, R. and D. percentages range from 2·5 to 0·6. If, furthermore, at 1961 prices the U.S. per capita G.N.P. back to 1954 and the corresponding R. and D. percentages, were superimposed on Diagram II, we would see the

[6] The degree of error is so large that the revision of 1954 R. and D. expenditure, bringing it to 1·4 per cent. of G.N.P., does not make a great deal of difference.

R. and D. percentage move from 3 to 1 per cent. with scarcely visible change in per capita G.N.P.

DIAGRAM II [7]



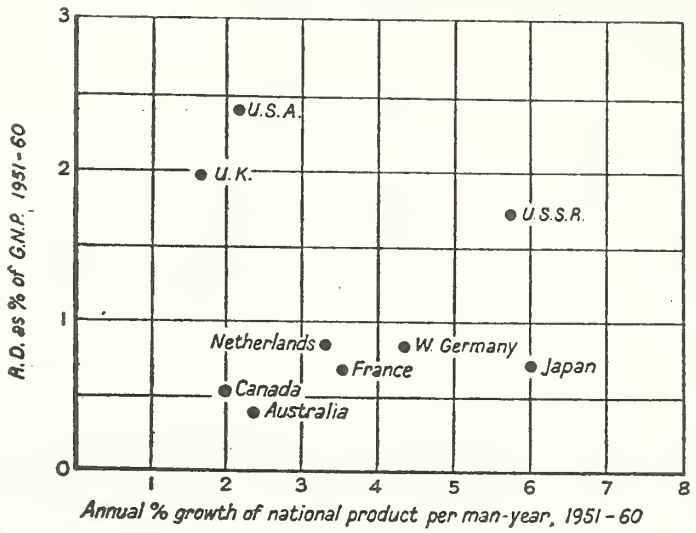
After this discussion it should come as no surprise that internationally there is no sign of a high correlation between rates of growth in output per head and the percentage of G.N.P. devoted to research and development. The effect of plotting the research and development percentage against the annual percentage growth of national product per man for nine countries is shown in Diagram III.

It may be argued that plotting research and development rates against growth rates for the same years is rather meaningless. This is a valid criticism (which should also be applied to Diagram I) if relative research and development rates have changed significantly. Research and development statistics in most countries are not very accurate but such evidence as there is suggests that lagging scrambled research and development figures five or 10 years behind the growth figures would not materially alter the picture.

In any case, we have very little clear-cut evidence about the appropriate time lags, which are not the same for research and development and probably not the same in different countries and over time. It is true that some research will have a direct impact on technology. Thus, successful research into optimum firing conditions in pottery could be applied directly to operating conditions in tunnel ovens. But for basic and background research and most industrial research, the impact on technology, if any, is

[7] From *Science, Economic Growth and Government Policy* (Paris: OECD, 1963).

DIAGRAM III [8]



routed through the development process. This may be a very expensive business—as, for example, the building of prototype nuclear reactors of different kinds at Calder Hall, Dounreay and Winfreth Heath. It may also be a lengthy business. Although nuclear reactors have been built for the Central Electricity Generating Board to operate commercially they are not in fact competitive with traditional forms of power. It is probably wise to regard the nuclear stations as part of the development process required to get that experience of building and operation which may make nuclear power costs just break even with, and then undercut, conventional forms of generation.

Recent expenditure on development is more likely to affect contemporary growth than equally recent expenditure on research. In considering the impact of research and development on growth this might not matter if basic research, applied research and development occurred in fixed proportions. But they do not. The actual proportion appears to vary over time and between countries. Due to difficulties of classification, estimates of the research and development proportions in different countries are subject to wide margins of error and the figures given below should, therefore, be treated with some caution. Nevertheless, the possible errors of estimation are not large enough to destroy the general picture.

[8] From Williams, B. R., *Investment and Technology in Growth* (Manchester: Statistical Society, 1964).

TABLE III

Estimated Percentage Distribution of R. and D. Expenditure in Four Countries [9]

		Basic	Applied	Development
United Kingdom	(1961)	11	25	64
United States	(1959)	8	22	70
France	(1959)	26	74	
Australia	(1961–62)	25	35	40

Research and development estimates are also scrambled in another important sense. The objectives of research and development also vary. It has been estimated that in the U.K. only 50 per cent. of research and development has a growth objective.[10] In so far as defence research and development (which until recently was over one-half the total expenditure in the U.S. and the U.K. and is now over one-third) contributes to growth it is in the nature of "fall out" or "spin off", which has not apparently been large. It is not then at all surprising that scrambled totals of expenditure on research and development have not shown a significant correlation with growth in national productivities.

Alternative Uses of Scientists and Technologists

It is now time to consider the implications of choice in the deployment of scientific manpower. Scientists and technologists are not only used in research and development. They are also used for the actual introduction and operation of new technologies. For example, in the chemical industry only about 50 per cent. of the scientists and 20 per cent. of the technologists employed are engaged in research and development. By contrast, in scientific instruments and electronic instruments the corresponding percentages are approximately 75 and 50. Given this sort of variation it is clear that the appropriate distribution of scientific manpower varies with industrial structure. But unless the distribution is appropriate and it would be very rash to assume that in each country it always is, growth potential will not be realised. From the need for scientists and engineers to introduce and operate sophisticated technologies there arises the possibility that research

[9] From *Annual Report of the Advisory Council on Scientific Policy 1961–1962*, Cmnd. 1920 (London: H.M. Stationery Office, 1963); Keezer, D. M., "The Outlook for Expenditures on Research and Development during the Next Decade", *American Economic Review*, L (May, 1963); *L'Usine nouvelle*, 18 May, 1961, p. 29; Williams, B. R., *Industrial Research and Economic Growth in Australia* (Adelaide: The Griffin Press, 1962).

[10] Carter, C. F., and Williams, B. R., *Government Scientific Policy and the Growth of the British Economy* (Manchester School of Economic and Social Studies, 1964), reprinted from *Manchester School of Economic and Social Studies* (September, 1964).

and development may be excessive. It will be excessive, that is to say will actually hinder growth, when the output of potentially usable new technology is greater than the capacity of industry to absorb and when the use of more scientists and technologists outside research and development departments would increase industry's capacity to carry through technological change.

The possibility of excessive expenditure on research and development is increased by the fact that research and development do not have to be home grown. Unless a country is leading in all fields of science and technology, there must always be some choice between making at home and buying abroad. Now the less advanced a country's technology and the smaller its supply of scientific manpower, the greater the advantage of importing science and technology and of using a higher proportion of scientific manpower outside research and development activities.

Consider the Australian case. Because the Australian population is very small there is no chance of an effective research and development effort in more than a small part of the field. Furthermore, the stock of scientists and engineers per head of working population in Australia is only half that in Britain. It follows that the real cost of using scientists and engineers in industrial research and development and particularly in research is, in general, much higher in Australia than in England. In fact Australia achieves a higher growth rate with a very much lower research and development percentage than Britain and the United States. Part of the explanation is that the Australian industrial pattern is different. If, for example, each industry (including agriculture) devoted to research and development the British proportion of net output, Australian expenditure as a percentage of G.N.P. would be only (defence complications apart) 60 per cent. of the British level. But this is not the main part of the explanation. The science-based industries in Australia—vehicles, chemicals, electrical—are dominated by overseas companies and this, by reducing the need for home-grown research and development, increases the effective supply of scientific manpower. If in those industries the expenditure on research and development were at the British percentage of net output, research and development expenditure in Australian industry would be about 200 per cent. greater than it is. As it is, the subsidiary companies are able to economise research and development by adopting or adapting the technologies of the parent companies.[11]

Of course, there are other ways of acquiring new technologies from abroad. Licences to produce patented processes and products and the purchase of " knowhow ", play important roles in the diffusion of technology. Sometimes licence terms, even the grants of licences, depend on the ability

11 See Williams, B. R., *Industrial Research and Economic Growth in Australia* (Adelaide: The Griffin Press, 1962).

to offer scientific or technological knowledge in exchange. But this is not the usual procedure. Thus, in 1961, the United States received from the recorded sale of licences and "know-how" $577 million, but paid out only $63 million. In 1963 Western Germany received DM200 million for patents, inventions and processes, but paid out DM550 million. Germany's payments for proven new technologies amounted to approximately one-sixth of its total expenditure on research and development, some considerable part of which will have no impact on growth. Clearly then the impact of foreign technology on German growth is even greater than the actual payment of DM550 million would at first sight suggest.

The following crude calculation helps to give some idea of the significance of this West German expenditure on foreign technological knowledge. British research and development is over $2\frac{1}{2}$ per cent. of G.N.P., the German just over 1 per cent. Now if we deduct the purely defence element in this and add in expenditure on foreign technological knowledge (multiplied by two in recognition of its proven impact on technology), the combined figure is approximately the same percentage of G.N.P. in both countries. If, as could well be argued is appropriate, a higher multiplier was used for expenditure on foreign "know-how", the German percentage would be higher. But through these differing combinations of home-grown and imported technologies in the two countries the deployment of scientific and technological manpower differs greatly. The stock of scientists and engineers as a percentage of the labour force appears to be no lower in West Germany than in Britain.[12] It follows that Germany uses a considerably higher percentage of its qualified scientists and engineers outside research and development than does Britain. There is nothing in West Germany's record of postwar growth to suggest that this policy (which many scientists regard as parasitic) has not paid handsomely. Equally, there is very little in Britain's record of postwar growth to suggest that her policy has paid off.[13]

[12] See *Resources of Scientific and Technical Personnel in the OECD Area* (Paris: OECD, 1963).

[13] The failure of our massive expenditure on research and development to produce a dramatic change in the economic position has produced a certain gloom in "the scientific world". But cheerfulness about the future keeps breaking in. For example: ". . . while spending on all R. and D. doubled between 1956 and 1962, spending on civil industrial R. and D. actually trebled. In this connection I would like to refer to a leader in *The Times* of 19 September, entitled 'Brighter News', which discussed the recent encouraging rise in British exports and output. The writer sought an explanation of Britain's improving competitive position in the restraint of costs at home and in the increasing wage pressures on the Continent. While these may well be important and essential components, I would myself be interested to know how much our improved position can be attributed to this rapid increase in expenditure on industrial research and development to which I have just referred. Certainly we would expect that this investment would now be beginning to pay dividends." Sir Harry Melville, "Industrial Research and Development in Britain" in *Science and the City* (London: Harrison, Raison & Co., 1963), p. 41. Since Sir Harry wrote this, the performance of industrial production and exports has been very discouraging.

The Significance of R. and D. Percentages

There is no inherent virtue in high R. and D. percentages, just as there is no inherent virtue in high rates of capital expenditure. They are both parts of the investment process and as such constitute costs of growth. Optimum allocation of resources involves taking investment in R. and D., as well as in plant and equipment, to the point where prospective yields to additional expenditure equal the cost of the finance involved. The existence of average yields to research and development higher than the cost of additional finance does not as such indicate under-investment. The crucial thing is the marginal yield, which may quickly fall below the average yield because of the limited rate at which production and marketing departments can absorb new knowledge, instal new processes and market the additional (or different) outputs. This rate is itself affected by the distribution of scientific and technological manpower within the firm (and even on occasions within firms expected to buy the new products). Given the low elasticity of the supply of scientists, the time required to absorb newcomers, and possibly the need to recruit less able scientists and technologists, marginal yields to research in fields where both the market and technological potential for innovation is high will usually be very much lower than the average yields.

What can be said about the R. and D. percentage at which marginal yields will become equal to the cost of finance? Is there some basic tendency for the R. and D. percentage to grow with per capita G.N.P.?

We have seen that there are very wide differences in R. and D. percentages in countries with similar levels of per capita income and that national R. and D. percentages are not closely correlated with growth. Nevertheless, in almost every country with a growing standard of living, the ratio of R. and D. expenditure to G.N.P. has shown a strong upward tendency.[14] What is this due to and how far can we expect the rise to go?

Leaving aside military R. and D., the growth of which in the last five years has fallen behind civil R. and D., we can impute the rise in the R. and D. percentage to three factors. Because manufacturing industry generally has a much higher ratio of research to output than primary and tertiary industries, industrialisation, which brings an increase in the relative importance of the manufacturing sector, usually pushes up the R. and D. percentage. This, however, is a transitional influence and does not apply to countries which are highly industrialised. In such countries, a rise in the R. and D. percentage is due both to a shift within the manufacturing sector and to the " discovery " of research in the traditional industries.

A shift within the manufacturing sector (implied in Diagram I) towards the research-intensive industries is the effect of three factors. The first

[14] See *Science, Economic Growth and Government Policy* (Paris: OECD, 1963), pp. 22–23.

is the effect of industrialisation in poorer countries on the industrial structure of the richer countries. This industrialisation undermines the international trade position of the richer countries and forces them to specialise more in those industries which require higher rates of investment in plant and in people.[15] On the whole these are the research-intensive industries—though, as we have seen, part of the required research and development can be imported. The second factor is the tendency for people in rich countries not to react to their riches by demanding more leisure rather than more goods. This makes it profitable to invest in inventing new commodities. The third factor is the effect of higher incomes on investment in people, including the training of scientists and engineers. Here, as in the case of R. and D., the relation is very general. Thus, the Russian stock of scientists and engineers as a percentage of working population is higher than the British despite a considerably lower level of income per head. This sort of variation is indeed an important factor contributing to the confused position pictured in Diagram III. But given this greater supply of scientists and engineers, the capacity of countries to invent potential new products and processes and to use them efficiently is increased, so raising marginal yields to investment in technology.

There is, however, no reason to expect this process to continue unchecked. For, although some of the new processes and products created by research and development do not require capital expenditure to bring them into effect, many, perhaps most, of them do. Even if, as there is no reason to believe, the supply of very creative scientists and engineers increased proportionately to the overall supply, limits to the rate of growth in capital expenditure would set limits to the growth of research and development. In the United States, Professor Yale Brozen has argued that the correlation between research expenditures and subsequent sales has tended to weaken since 1957 and this is a sign of research and development expenditure moving nearer to an equilibrium situation. In 1960 he predicted that the portion of national product devoted to research and development would rise from 1·9 to 3·0 by 1975 with a ceiling of 5 per cent. in the next century.[16] His 3 per cent. has arrived a decade early, partly because, like Ewell, he overestimated the growth of national product. This is an important point, for nothing pushes up research and development percentages faster than a failure to achieve the growth expected from research.

[15] See *Industrialization and Foreign Trade* (Geneva: League of Nations, 1945).

[16] Brozen, Yale, "The Future of Industrial Research and Development" in *The Rate and Direction of Inventive Activity*, pp. 273–276.

What Should We Expect from Research

Countries differ in levels of income, in levels of technology, in industrial structures, in size, in the proportion of scientists and engineers in the work force and so on. These differences affect the R. and D. percentages at which the yields on additional research fall to a low level; the ratios of home-grown to imported science and technology; the appropriate distribution of expenditure between basic and applied research and development and of manpower between research, development and other activities.

In a country such as the United States, with a relatively high proportion of scientists and engineers in the work force and a technological lead in most fields, a high R. and D. percentage will be consistent with an attempt to achieve high growth and a condition of it. A high R. and D. percentage will not leave a critical shortage of men required to instal and manage new technologies. And given the restricted opportunity to borrow new technologies from abroad, a big development effort will be required to create the new technologies. As the country gets richer there is likely to be an increasing struggle to maintain a high growth rate as trade positions in existing export products are weakened by the diffusion of its new technologies abroad and higher investment rates are required to establish the still newer technologies. Just how all this will affect the total relations between R. and D. percentages and growth will depend on a very large number of factors, including trade policy, population growth, military expenditures and future income elasticities of demand for leisure. If, for example, the demand for leisure increased, we could expect relatively greater emphasis on labour-saving process innovations, which would have a bigger effect on product per man hour than on product. In other words, product per man hour could grow rapidly, even though G.N.P. did not.

Between the countries of Western Europe and the United States there are important differences, which should affect what we expect from research. The fields where any country has a technological lead over the U.S. are few and in all countries the stock of qualified manpower is a smaller percentage of the work force than it is in the U.S., and very much smaller anyhow. It follows that no country can afford to try to cover the whole field of industrial research and development [17]; that any such attempt must lead to a weak effort and, therefore, a low R. and D. yield in all fields. It follows too from the possibility of importing technology from the U.S. —whether in the form of payment for technical knowledge or the willingness to allow the operation of American subsidiaries—that the dependence of

[17] Indeed, in some cases even one field is too expensive. Hence the collaboration between France and Britain in developing the Concord and between the Common Market countries in the development of nuclear power.

innovation on home research and development is less than in the U.S.[18] Just how far it will pay a West European country to import technology will depend on the cost of acquiring it and the efficiency of home R. and D.[19] It is thought, for example, that the recent rapid rise in Japanese research and development has been connected with a rather steep rise in the cost of buying " know-how ".

What follows about the distribution of qualified manpower between research and development and other activities is not so clear. For, as we saw in the case of Australia, the use of foreign subsidiaries in research-intensive industries has the effect of adding to the effective manpower supply. But the probability is that the most economic use requires a smaller proportion of scientists and engineers in research and development than in the United States.[20] On the other hand, purchasing " know-how " and giving foreign firms with advanced technologies facilities to operate, may have a greater economising effect on development than on applied research. The precise effect will, of course, depend on the fields in which most use is made of foreign development work. But certainly the effect could be to produce more research in relation to development than would be sensible in a closed economy.

[18] This is one reason why correlations between R. and D. and profits are higher in the U.S. than in Britain.

[19] For an account of the great impact of U.S. firms on British innovation see Dunning, J. H., *American Investment and British Manufacturing Industry* (London: George Allen and Unwin, 1958).

[20] Some of the implications of all this for government scientific policy in Britain have been examined in Carter, C. F., and Williams, B. R., *op. cit.*

Reprinted from "Report to the President -- The President's Commission on Heart Disease, Cancer, and Stroke. Volume II, February 1965. By the U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE -- Public Health Service

Source Paper

CONFERENCE ON THE ECONOMICS OF MEDICAL RESEARCH

At the request of the President's Commission on Heart Disease, Cancer, and Stroke, Dr. Walter Heller, Chairman of the Council of Economic Advisers to the President, called together a group of economists for a meeting on September 30, 1964, to discuss some of the economic aspects of medical research.

A series of questions served as points of departure for the day's discussion. These were initially submitted by the Commission and elaborated in detail by the Council of Economic Advisers. They are contained in the appendix.

Participants in the conference were:

Economists

Kenneth J. Arrow, Professor of Economics and Statistics, Stanford University.

Peter de Janosi, Program Associate, Ford Foundation.

W. Lee Hansen, Staff, Council of Economic Advisers.

Walter W. Heller, Chairman, Council of Economic Advisers.

Herbert E. Klarman, Associate Professor of Political Economy and Professor of Public Health Administration, The Johns Hopkins University.

Dorothy P. Rice, Medical Economist, Health Economics Branch, Division of Community Health Services, U.S. Public Health Service.

Tibor Scitovsky, Professor of Economics, University of California.

President's Commission

Stephen J. Ackerman, Executive Secretary.

Michael E. DeBakey, Chairman.

Edward Dempsey, Special Assistant to the Secretary (Health and Medical Affairs), U.S. Department of Health, Education, and Welfare.

Mike Gorman, Consultant.

William Kissick, Staff Assistant.

Abraham M. Lilienfeld, Staff Director.

John D. Turner, Staff Associate.

The following is a report of the conference.

The Agenda

The chairman of the Council of Economic Advisers convened the meeting in behalf of the President's Commission on Heart Disease, Cancer, and Stroke. The Commission had asked for some guidelines on what constitutes a reasonable outlay for medical research in general, and particularly research in heart disease, cancer, and stroke. This type of problem is still on the frontiers of economic knowledge and research. Economics has made a fair degree of progress, especially since World War II, in developing criteria for investment in relatively intangible types of activity, such as education. Whether it has progressed to the point that it can provide concrete guidance for medical research expenditures expressed in quantitative terms is questionable.

The concerns of the President's Commission were expressed by its representatives as follows. Medicine has made great inroads in attacking these diseases—heart disease, cancer, and stroke. Even with the knowledge possessed at the present time, medical care can make a great deal more progress. There are many types of

heart disease that can be virtually cured today by certain forms of treatment, including surgery. We do not yet know how to affect the course of coronary artery disease. Conceivably there are ways by which some of these diseases may be overcome completely, since the heart performs a mechanical function that can be substituted for by mechanical means. We are able to substitute completely for the heart function in humans for several hours. We can do this in the laboratory on experimental animals for days, with the animals surviving and performing normally in every respect. We cannot do this for longer periods today for mechanical reasons largely. If this problem were solved, the survival level and the productivity of the population would be raised substantially, since heart disease accounts for more than 50 percent of all deaths.

To make the inroads envisioned, funds will be required not only for research, but also for the support of trained manpower, facilities, construction, and so on. The Commission would like to see the health field in as advantageous a position as possible to compete for funds. When a group of diseases costs the Nation large amounts of money, it seems reasonable that they would be assigned a high priority. What is not clear is how one relates the importance of health to other types of national concern and activity, such as defense.

The Commission's representatives also expressed interest in the allocation of resources to patient-care services. This item is additional to the original agenda, which was limited to medical research. Accordingly, they invited discussion of criteria for allocating funds to research, training, and patient care, as well as to research expenditures by disease category. Would it be legitimate to allocate expenditures among diseases according to their respective contributions to mortality? To medical care expenditures? To loss of output?

Another way of stating the Commission's concern is this: Suppose one knew the annual impact on the economy of these three groups of diseases. Assume that medical care expenditures and loss of output due to mortality and morbidity amount to $8 billion annually (as shown by the preliminary data for the year 1962 compiled by the Health Economics Branch of the U.S. Public Health Service). Can economists say how much the Nation can afford to, or should, spend on research and on patient care in these diseases? How does one take into consideration the rising productivity of labor, the growing gross national product (GNP), and so forth?

QUESTION 1: How much can this Nation afford to spend, or how much should it spend, on medical research?

It seems doubtful that a population has any value judgments or feelings about medical research which are independent of its value judgments or feelings about health. If so, the basic question concerning any proposed expenditures in the health field is whether the resulting output will make a commensurate contribution to the health of the population.

Economists hold that it is not meaningful to ask, How much can this (or any) Nation afford to spend on medical research. The real issue is how much it is worthwhile to devote to this purpose in comparison with other purposes, given the expected respective returns from the same resources.

In addition to the size of the disease problem, however that may be measured, there is the uncertainty of return from any research expenditure. It is important to ask whether or not a significant step forward is imminent.

The availability of specialized resources apart, the proper question consists of three elements:

a. What is the estimated value of improving health services—in terms of reduced mortality, reduced morbidity, etc.?

b. What is the probability that this value will be realized through increased expenditures on research?

c. How does this value compare with the estimated cost of achieving it?

It is well to recall that included in the customary calculations of the economic costs of a disease (and the benefits of eliminating or reducing it) are loss of output and medical care expenditures. Other costs, such as pain and

grief, are usually neglected because they are difficult to measure.

The economists agreed that the cost of granting relief (public assistance) to support a disabled person, who may constitute a burden to society for a long period, must not be counted separately. That would be double counting, since the loss of his output has already been measured.

Economists have evidently made greater progress in measuring loss of output due to morbidity and premature death than in measuring the value to the individual or his family of avoiding pain and suffering. The question arose whether the attempt to measure the latter does not entail double counting. The answer is that there is double counting if one simply asks how much individuals are willing to spend on medical care, when the anticipated outcome includes the continued or restored ability to work. However, if one carefully tries to ascertain how much individuals are willing to spend beyond any effects on earnings, the danger of double counting is effectively precluded.

The representatives of the Commission pointed out that the greatly increased expenditures on medical research in the postwar period have produced much knowledge that is currently being applied to the care of patients. There has not been a single great advance in medicine in the last 25 years that is not attributable to expenditures on medical research. Without the products of research, physicians would be rendering today the same kinds of service they rendered in the past. In the last 15 years alone many of the congenital forms of heart disease, which formerly were completely hopeless, have become curable. In most instances they led to early death and in a majority of instances to a high rate of disability on the part of those who reached adult life. Today the majority can be cured by surgery. This is entirely the product of medical research.

The Commission's representatives introduced several additional examples. In the past a victim of stroke received a small amount of medical care. His destination and resting place were the nursing home, because medicine did not know what to do for him. Today a majority of all imminent stroke patients can be treated effectively by surgical means.

With present knowledge, early detection of cancer of the cervix would probably yield a 70 percent decline in mortality within a short interval. Rheumatic fever and rheumatic heart disease have become manageable by means of antibiotics, and surgery can be used to treat the resultant heart damage when it occurs.

The economists suggested that efforts be made to quantify the returns from medical research. By definition research is a leap into the unknown, and the only basis for guessing about the probable outcome in the future is what has happened in the past. This is true of all types of research. Only one attempt has been made to estimate the economic return from investments in research, namely, in hybrid corn. The yield proved to be of the order of several hundred percent per annum. It seems reasonable to suppose that a research project of modest magnitude would lead to a determination of the return from research that produced the modern treatment of congenital heart disease. Examples in other diseases will readily occur to the investigator. In performing such a calculation, needless to say, one should consider everything, failures as well as successes. It is plausible to assume that too successful a record in medical research ventures—say 50 percent—reflects insufficient expenditures.

The economists agreed that if the procedures developed by medical research prove to be costly to apply, the potential net gain is reduced. This fact will always be reflected in the comparison of expected cost and gain.

The gains due to research are not direct, but are entirely attributable to the effects of services on the health of a population. The provision of services and the effects of services are intervening steps. When the services are not rendered, there are no gains. If the effects of specific services on health are not known, the value of medical research cannot be known. Unfortunately, knowledge of the effect of specific services is often lacking, as for example in coronary heart disease.

The Commission's representatives suggested that since prevention is preferable to cure, research expenditures on preventive measures

may deserve priority. To an economist this conclusion is not evident. It is likely that the return from prevention exceeds that from cure, when each is equally attainable. Usually, however, they are not equally attainable. Conceivably the probability of developing effective preventive measures may be low, while the probability of developing helpful curative measures may be high. If so, the latter deserve priority. It is an empirical question. In considering any proposed expenditures on medical research, therefore, the probability of success is an important consideration.

Other things being equal, however, one expects to devote more economic resources to problems of larger size (as measured by the value of the economic loss), because the cost of research is more or less independent of the size of the problem while the value of the return is proportional to it. Presumably, it would cost the same amount to prevent one death through the output of research as to prevent a thousand deaths. The yield of research is a strong example of the operation of economies of scale.

The representatives of the Commission restated the initial question. Given what is known about the size of the economy today and its expected size 5 years from now; given the level of expenditures on medical research; and given the magnitudes of the three disease categories, can economists advise the Commission whether or not the Nation's research endeavor should be expanded?

The economists regretfully expressed their inability to do so. To do so responsibly would require certain kinds of data as a basis for a series of calculations. It is conceivable that very simple calculations will show that increased expenditures are warranted. The size of the economy and the magnitude of the disease problems are elements in the calculation, but other elements enter also. Least important perhaps is the size of GNP, because medical research expenditures are not a large fraction of the total. Obviously the resources exist in this country to permit increased expenditures on medical research. What is important are the probability of success of a given research undertaking and the value of the benefit, if realized. The product of the two factors is the expected value of the research, which must then be related to the cost of this research. Even a small improvement in the probability of success (say 2 percentage points), if it applies to a sufficiently large base, can yield a large gain.

The same is true of a small reduction in the base, especially if the gain is realized in perpetuity. If a given research expenditure could yield an annual reduction in cancer or heart disease deaths of 3 percent, the economic gain would be great. The question is with what probability a reduction can be attained at a given cost.

The economists introduced the concept of time and the discount rate. Both benefits and costs may occur at various times in the future. A dollar today is worth more than a dollar 10 years hence. The discount rate renders these dollars commensurate. The sum of a stream of dollars in the future, properly discounted, is the present value of that stream.

With regard to the initial question, an economist simply would not ask, "Can the Nation afford it?" He would ask, "Is it worthwhile to spend an additional sum on this objective rather than on something else?" To answer this question he will estimate the expected value of a research project (as reflected in diminished mortality and morbidity, and the value attached thereto) and compare it with the cost. If a given amount of money is available for spending by Government, say $100 million, it is necessary to ask whether adding this sum to medical research expenditures would yield better results than adding it to expenditures on education. To the extent that both health and education provide sheer consumer satisfactions, the outcome of the comparison will depend on individual value judgments; to the extent that they both contribute to the GNP, the comparison can be made somewhat more objectively.

The economists offered a reformulation of the "worthwhile" question that may be easier to deal with. A given sum of money spent on research would be equivalent to a 5-percent return on investment if it led to a specified probability of reducing mortality by X and morbidity by Z.

The economists noted that what is always at issue is the marginal or additional dollar, not total expenditures. Everybody would agree

that it would be disastrous to abolish all research. It is conceivable, however, that at a given time and place, we may be scraping the bottom of the barrel for problems worth investigating, as well as for talent capable of working on them effectively. Thus, under specified conditions, it may pay to spend $250 million on research, but it may not pay to spend an additional $100 million, or a total of $350 million.

The question was raised whether the criterion for an optimum expenditures policy on medical research could not be something other than the maximum difference between the present values of costs and benefits. Why not adopt a policy of incurring large medical care expenditures, while minimizing indirect costs due to morbidity and mortality in the productive ages? The answer is that the first policy is the correct one, if all the elements are included in the calculation and properly measured, including imputed values for intangible items (see question 4).

The point was also made that even a successful research venture may have a low value ultimately, if the incidence of the disease in question is low. Under these circumstances a screening device of high reliability produced by the research would still yield a high proportion of false positives.

The economists saw no difficulty here. The high costs of rendering effective service with the new screening device enters into the calculation. Direct costs will be high, offsetting the decline in indirect costs. Increased expenditures required to disseminate knowledge also will entail increased direct costs.

A related problem is that the available knowledge is not always applied to reduce mortality and morbidity. There is a gap, then, between the productivity of research and its payoff in practice. It is easier to secure adoption of measures that can be applied on a mass scale than of procedures that require administration to individuals. One of the economists suggested that consideration might be given to calculating benefits in terms of two types of dollar: For example, a 100-cent dollar for research findings that are likely to be applied widely and a 50-cent dollar for findings that are not so likely to be applied.

QUESTION 2: Are there any economic criteria for determining the proper roles of the several levels of government in financing medical research?

In general one would expect a relative shortage of private support for medical research, because the returns from certain types of medical research cannot be captured or appropriated. There is broad agreement among economists concerning this proposition and its corollary, the need of support from public funds—philanthropy or taxes.

Related, though not identical, grounds for support from public funds are the following characteristics of research as an economic good:

1. Its "public goods" aspect: It is an economic good of which the total supply is available to any one individual. "More for you means no less for me." A common example outside the health field is the lighthouse.

2. The presence of external effects: The value of medical research to society or a group is greater than its value to a single individual. In this case reference is made to the furtherance of research through the dissemination of existing knowledge. Another dimension of externality is geographic, namely, that application of the product of medical research cannot be confined within a given geographical area.

3. Economies of scale: Unit cost declines as the volume of output increases. One aspect here is the fact that the reproduction of research is cheaper than the initial cost of producing it. Also, knowledge is cumulative in character and each new building block may be decisive for a breakthrough. Finally, reference has been made to the economies of scale on the benefit side.

4. Uncertainty: The probability of success as an important element in the calculation has been mentioned. Where uncertainty looms large, as in medical research, it pays to diversify risks. This is done by undertaking a large variety of projects, some of which will be successful. Accordingly, total expenditures must be large.

It is reasonable to suppose that unless the re-

turns from expenditures on research are appropriable, private business will not ordinarily incur them. The product of basic research is usually not subject to appropriation. Some products of applied research can be, and are, appropriated as in the case of drugs, which are protected by legal grants of patents for a limited length of time. The dangers here are twofold: (*a*) The guarantee of monopoly provided by a patent may be perpetuated by small changes in the product; and (*b*) a substantial change in the product may not always be compatible with the health of patients. There is some evidence that a proliferation of new drugs has occurred, some of which add nothing useful to the physician's armamentarium.

In summary, one cannot rely on the profit motive to bring about a sufficient flow of private funds into medical research. Therefore, Government must fill the gap, if the public is to obtain the benefits of medical research. Indeed, the question was raised whether as a matter of public policy, Government financing should not be extended into some areas of medical research whose benefits can be privately preempted.

The Commission's representatives observed that although the amount of money spent for medical research by nongovernmental sources has increased, the amount spent by government has increased even more, with the result that the nongovernmental share of the total has declined. There are those who believe that the very increase in expenditures by government will serve to reduce private support. The answer to this argument, it seemed to the Commission's representatives, is that several large philanthropic foundations have undoubtedly moved into other areas of activity, in furtherance of their mission to explore new fields and to develop new programs, leaving to government those activities that no longer represent the new or the experimental. Whatever the reasons may be for the shift in spheres of concentration on the part of the foundations and whatever the justification, the need for funds for medical research exists. If the foundations will not supply the funds, government must.

The economists stated that in this case government necessarily means the Federal Government, for State and local tax funds are unlikely to fill the need. Not only are the States and municipalities short of revenues to discharge all their pressing obligations, but they lack compelling reasons for devoting their resources to medical research. Indeed, the high rate of mobility in the population of this country and the inability to localize the application of medical knowledge means that the benefits of research will not accrue exclusively to the residents of any small area who pay for it. The major exception is an unusually high incidence of a disease in an area or a highly localized disease, as exemplified by air pollution in Los Angeles.

The economists thought that the externality factor associated with geography was so important, that States and local units of government might be reluctant to pay for medical research even if their fiscal capacities were substantially enlarged. The wisdom of the city of New York in appropriating funds for medical research has been questioned.

One economist present believes that financing a major portion of medical research from Federal funds is also justified on grounds of equity. This is akin to the feeling most people have in favor of distributing medical services in relation to need, rather than ability to pay.

The other economists thought that equity is based on broader considerations than the financing of a particular service. Moreover, while people may have strong feelings concerning the ready availability of medical services, regardless of individual ability to pay for them, it seems doubtful that they have any feelings, much less strong ones, on who should pay for medical research.

One of the Commission's representatives observed that medical research and medical services are intimately related, for the provision of services will suffer if medical research does not continue. If considerations of equity enter into the provision and financing of services, they must enter into the financing of medical research. Medical services are merely the delivery of the end product of medical research. Moreover, some of the needed programs of service in

heart disease, cancer, and stroke will not get underway, in light of past experience, unless Federal financial support is forthcoming.

On this point there was also disagreement among the Commission's representatives, and no consensus was reached.

QUESTION 3: Are there criteria to guide the allocation of funds between general and specific medical research?

At a meeting at the National Institute of Mental Health in 1963, it was brought out that three of the major advances in the treatment of the mentally ill, which have served to reduce the load in the State mental hospitals, have come from outside the field of psychiatry or psychiatric research. Penicillin has virtually eliminated the late complications of syphilis, including paresis. Vitamin B complex has eliminated pellagra. Tranquilizers were developed in research on cardiovascular diseases. It would seem to an outsider, therefore, that research in specific fields does not always yield the results expected of it.

The Commission's representatives explained the rationale behind the categorical approach to medical research. Individuals have specific, not general, diseases, and medicine deals with specific diseases. Medicine is organized by disease categories (specialties), because this is the most expeditious and most effective way to deal with diseases. This pattern of organization also serves to promote more rigorous research, because concentrating on a specific area of disease tends to sharpen the focus of investigators. Although an important contribution to services in one area may originate in research in another area, most major findings in medicine are the product of categorical research.

There are additional reasons for singling out stroke as a field of research and patient care. In the not distant past, stroke was almost totally neglected, because it seemed a hopeless situation, and few people were conducting research on the disease. There is also the high correlation between the incidence of stroke and age, and our society lacks real interest in the aged. Quite recently new knowledge regarding stroke has emerged as a byproduct of technical advances in radiology. Today one can be optimistic about the returns that will accrue from increased research expenditures on stroke.

Already a great deal can be done to prevent the occurrence of stroke and to help the patient recover from many of its effects. Today the vast majority of stroke patients are still treated by rehabilitation. But recently there has developed the potential of diagnosing impending stroke on the basis of certain signs and symptoms, leading to the application of surgery to prevention. This service is currently available in only a few medical centers, which report excellent results. Two separate groups of physicians deal with these problems, and their facilities are distinct. To stimulate and maintain interest in this disease entity, a categorical approach is required.

The Commission's representatives invited attention to another recent development. In each of the special disease areas, the concept of a multidisciplinary approach (from both the biological and physical sciences) to a particular disease has been widely adopted. Therefore, the new institutes and specialized centers are not nearly so narrow as supposed.

In the discussion of Question 3 the economists were pursuing the implications of a second type of uncertainty, namely, one's lack of knowledge of the disease area in which the returns from medical research expenditures will accrue. Under these circumstances how feasible are calculations on the economic worth of research expenditures in that specific field?

The Commission's representatives replied that there will be no medical progress whatsoever in the absence of expenditures on medical research. Among the issues facing the Commission are: How much more money should be put into medical research in general; how much money should be put into research specifically related to heart disease, cancer, and stroke; and how much money should be put into applying the knowledge gained from research? To be specific, can the fact that 22 percent of all health research expenditures are in the fields of heart disease, cancer, and stroke, while 71 percent of

all deaths are attributable to these diseases, serve as a basis for raising the former proportion?

The economists stated that the answer to the specific question is, No. To begin with, the share of research funds allocated to the three disease categories should be based on all categorical research funds, not on total research expenditures. The denominator should be reduced by deleting general research expenditures.

Second, the comparison of research expenditures should be with the economic value of the diseases, not with deaths alone, or the value of deaths alone.

Finally, cognizance must be taken of the probability of success in a particular research undertaking.

Perhaps a more satisfactory and more constructive approach could be taken along the following lines. Suppose applications for research support fall into a number of classes—one general and the others categorical. Why not examine the quality of the applications that are not funded in each class? There may be no objective basis for making such a comparison, but an expert in medical research may be able to state after a review of applications that in his judgment projects being rejected in one class are markedly superior to those rejected in another class. Obviously an economist cannot make this judgment.

The Commission's representatives pointed out that the probability of success in research is not merely a function of the state of knowledge. Psychological and other intangible factors are also involved. Certain areas of investigation attract talent, because Nobel prizes have been awarded in them. Money is sometimes needed to serve as a counterweight, at least initially, in developing an area of research.

In view of the uncertainty concerning the disease area in which the return, if any, from a research expenditure will occur, the general approach to research would appear to the economist to be more logical. Perhaps a review of the evidence would be helpful: How often do returns take place in fields other than those intended?

One can, however, make a case for the categorical approach to research on several grounds. If decisions are difficult to make, one way to facilitate the process is to simplify it and one way to simplify is to establish several arbitrary classes within which all research projects fall. Another consideration is strategic, pertaining to the most effective way to obtain appropriations. Congressmen know about specific diseases. Still another point is that the more categorically oriented a project is, the closer it is likely to be to problem solving than to basic research. A research category should be as broad as the medical specialty within whose purview it falls, the Commission's representatives observed.

The consensus of the economists was that one seems to be dealing here with questions that are largely strategic and administrative in nature. Although economists may have opinions, economics as such has little to say about such decisions.

QUESTION 4: How should one handle certain complicated aspects of the economic calculation, such as the value of pain and grief, the implications of interrelated diseases, and failure to apply new knowledge?

The economists stated that the relief of pain and suffering is truly an economic good to which people, by their actions, ascribe a value. This value in avoiding illness and disability is additional to that of the economic ouput of a person who is well and working. To escape serious illness a person is willing to spend more on medical care than the sum of the averted loss of earnings plus anticipated savings on future medical care expenditures.

There is also a value to human life that goes beyond the survivor's contribution to economic output. It is recognized that it is not easy or simple to measure the value of a human life. However, to avoid the attempt at measurement is in effect to set the value at zero. That is obviously a mistake.

One way to measure the value of pain or grief is to examine what people actually spend in order to prevent disability or to save lives. This

provides an indication of their own implicit valuations. The elimination of railroad grade crossings is a good example of a public policy that, in part at least, is based on compassion. Expenditures on grade crossings per human life saved represent an upper bound to the value of compassion.

More explicit valuations are schedules of awards for injuries covered by workmen's compensation, schedules set by insurance companies for other purposes, and verdicts by juries in cases of disability or death caused by accidents.

Under specified circumstances, medical care expenditures actually incurred can be taken as a measure of the value of a certain benefit, which is apart from, and additional to, any prospect of avoiding loss of earnings or of reducing medical care expenditures at a later date. The benefit is that of alleviation of pain when the disease is known to be terminal, as in cancer, and no other economic benefits may reasonably be expected. By analogy the benefit and the expenditures by which it is measured can be regarded as the consumer benefit of avoiding the disease under study.

One difficulty with this process of valuation, it was noted, is that out-of-pocket expenditures are sometimes only a small fraction of a person's medical care expenditures. The remainder of the bill was paid earlier in the form of a general health insurance premium, and does not reflect the value placed on a particular service at the time of delivery. Out-of-pocket expenditures would constitute an understatement of the value, while the total bill might be an overstatement.

Nevertheless, this general formulation is valid: Suppose an individual were fully protected against earnings loss and all of his medical expenses were met by taxes; he would still be willing to spend some money to avoid illness, or disability, or premature death. Economists may differ on how to ascertain this sum of money, but they agree that it is greater than zero. Loss of output alone, or even in combination with medical care expenditures, understates the size of the economic loss attributable to a disease.

A serious research effort to measure the value of this intangible consumption benefit would be in order. It should be recognized, however, that precise answers cannot be expected.

As for interrelationships among diseases, there is no doubt that a person with two diseases has an increased risk of dying from either one of the diseases. For example, if a person has a bleeding ulcer and a weak heart, he cannot be operated upon, and the probability of his dying from the ulcer increases.

This is different from the matter of selection for survival. By the selection hypothesis we mean that people who formerly died from infectious diseases no longer do so and survive to die from diseases of the older ages. One implication of the hypothesis is that those who are saved are increasingly susceptible to disease in adult years. This is the argument advanced by Frangcon Roberts, the Welshman, who holds that all we are accomplishing is spending more and more money to care for sicker and sicker people. However, nobody really knows this to be so.

The economists asked the question: Why have advances in knowledge had so little effect on life expectancy at all ages other than infancy. One answer is that persons who survived to age 20 were always reasonably adapted to deal with their own germs. Another answer is that perhaps owing to changes in "living conditions," there has been an increase in lung cancer, in coronary heart disease, and in chronic bronchitis, which may have served to offset the declines in death rates from other causes. Still another answer is that applications of knowledge on a mass basis have yielded success at infancy. To deal with diseases in the older ages requires the application of knowledge on an individual basis, which is much more difficult to accomplish. In rheumatic fever, for example, the necessary knowledge exists, but it is not being applied. It is necessary to convince individuals to receive prophylaxis. If a pill were available to be thrown into the water supply, many more persons would receive prophylaxis.

Here the economists drew on an analogy. The profession usually takes the position that it is up to the economic system to provide jobs. A cost-benefit calculation for a disease control program assumes full employment. Similarly, it

may be equally valid to argue that if medical research produces the knowledge that can reduce cancer by x percent, provided that smoking is curtailed, then the research has been successful. Failure of the public to apply the new knowledge is another matter, which may deserve separate consideration.

The economists agreed that with the data currently available, it is not possible to prepare economic guidelines for the allocation of funds to medical research.

It is recognized that the return on medical research is extremely difficult to measure. Accordingly, it is highly important that we base our decisions not on the greater feasibility of measuring certain effects, but rather on what the measurements would show if all effects were equally measurable.

QUESTION 5: What can be done to bring together the Federal Government's interests in medical research and in educating and training personnel?

One of the economists present advanced the proposition that investments in training for medical research and in research should be phased, with the former preceding the latter. In actuality, the Federal government has supported medical research, in preference to training.

An objection to this formulation is that it is not possible to train individuals for research apart from the performance of research. Are these not complementary activities at the graduate level of training? It is recognized, however, that formal academic training in subject matter and methodology can with advantage precede participation in a research project.

The economists pointed out that whether a physician cares for patients and disseminates the knowledge that exists or does research is an important economic decision for the country. The Federal Government affects this decision by providing funds for research.

The issue can be mitigated, however, by increasing the total supply of physicians. True, this takes place at the expense of something else, but perhaps this lies outside the health field.

One economist present reported that the United States stands alone among eight countries in having a declining ratio of physicians to population. One result is a reduction in the availability of medical services at night, weekends, and at home. Another is a rise in prices. It was noted, however, that, though declining, the U.S. physician to population ratio is still second or third highest among all countries. The fact remains, of course, that relatively little is known about the effect of some services on the health of a population.

Beyond the general issue of a limited supply of physicians for all purposes, there is another issue, that of the availability of personnel for research. Physicians who do research are not necessarily better, but they are specially qualified. Economists find it difficult to believe what is widely accepted in the health field, namely, that service, education, and research are so closely intertwined that a good physician can do all three with equal ease and competence.

The question was raised why a high-income profession like medicine should require the inducement of fellowships to attract recruits? One answer is, in order to match or overcome the attraction of physics and molecular biology. This may not be necessary, however. Biologists and physicists perform a good deal of medical research today. They have Ph.D. degrees, rather than M.D. degrees.

QUESTION 6: Can economists offer any guidance on the respective merits of project vs. program research financing?

The initial reaction was that there was no reason to make the choice all one way or the other.

One of the economists present set forth two propositions. One, in the form of a question: Is there ever a time when the Federal Government can say that the Nation needs a particular piece of research and then go out and solicit

grant applications? If so, there is a clear case here for project support. Moreover, it would not seem to matter whether the method of payment is a research grant or a contract. Indeed, there is no reason why the contract mechanism cannot be combined with scientific review.

The other proposition is: If one always allows the investigator to apply for funds and the Government sets no priorities on problem areas to be investigated, then it would seem that a strong argument may be made in behalf of program support over project support.

The reason is that the method of financing research affects productivity. Under project financing there is instability of tenure; the writing of progress reports; the filing of applications some time before the current grant expires; and the tendency to set out to compile new data under a new grant, rather than conducting additional analyses of the data gathered under the old grant.

There was no consensus on these issues, in the absence of concrete knowledge of how the grants mechanism is operated and performs in practice.

Numerical Estimates of Economic Costs of a Disease

As part of the work of the President's Commission, the Health Economics Branch of the Division of Community Health Services of the U.S. Public Health Service was commissioned to measure the so-called direct and indirect costs of heart disease, cancer, and stroke. In view of the limitations of time, staff, and the state of the art, the Health Economics Branch defined its mandate as follows:

a. Calculate the medical care expenditures incurred by, or in behalf of, persons with heart disease, cancer, and stroke in the year 1962. Primary diagnosis (the final diagnosis for the condition that brought the person to medical attention) is the basis for classifying persons and their expenditures (or costs incurred in their behalf).

b. Calculate the loss of output by persons who died of, or were disabled by, these diseases in 1962.

Preliminary figures were prepared by September 30 and were distributed at the Conference. The results of the discussion may be summarized as follows:

Single year estimates of loss of output seriously underestimate the size of the economic problem.

Ideally, to help in policy formulation, the calculation should represent the present value of the future losses in output due to premature death in 1962. A less desirable alternative, but still a marked improvement over the single year estimate as a measure of the economic magnitude of a disease, is to calculate the loss of output in 1962 due to premature death in previous years by those who would have been alive and productive in 1962 if heart disease, cancer, or stroke had not intervened and caused them to die.

Looking toward the future, a social rate of discount should be applied. A discount rate is obviously not applicable to the losses attributable to past deaths, because all output is measured in the year 1962. The estimate oriented toward the future should allow for expected increases in the productivity of labor.

If it were known how many persons became permanently disabled in 1962, the same procedures would be applicable to morbidity as to mortality. In general, the most useful estimates for policy purposes would represent the present value of future losses in output attributable to the occurrence of initial episodes of disease in a specified interval. The basic data for preparing such estimates are usually lacking, however.

The same is true of medical care expenditures. Persons who suffered a heart attack in 1962 and survived may have expenditures in the future. These should be discounted to the present in order to calculate present value. Again the necessary data are lacking.

With one or two or three diseases taken at a time, one cannot be sure that the calculations of costs are performed consistently and correctly. To be assured of this it is necessary to perform a complete accounting for all catego-

ries of disease, however classified, at the same time. This appears to be a practicable procedure in measuring direct costs (medical care expenditures), where the validity of the procedures and the accuracy of the calculations can be confirmed by comparing the results with known totals for a single year. No such check is available when the present value of future earnings is calculated. For indirect costs, moreover, existing data are so much more fragmentary and unreliable as apparently to render the desirable step impracticable.

Questions were raised about the adequacy of the classification by primary diagnosis alone, but no decisions were reached or recommendations made.

Causes of death may not be truly independent. If so, the elimination of a given cause of death from the life table represents the maximum reduction in deaths that is attainable. The true gain is probably lower.

Several diseases frequently coexist in a patient. This means that to attribute the total costs to any single disease is to overstate the potential gain from overcoming it. Consistent adherence to classification by primary diagnosis has the advantage of counting a person only once. If diseases occur independently (which they do not), the overstatements and understatements will more or less balance.

It was recognized that the task undertaken by the Health Economics Branch was laborsome and complicated. Given the time limitations, it was well performed. Some of the desiderata stated above could only be incorporated in another future project.

Problems for Research

It is useful to isolate and present some of the problems that were proposed for future research.

1. What has been the return on investment in specific research projects? How often has the return accrued in the categorical fields that sponsored the research?
2. In calculating the present value of future losses and gains, take into account the losses due to future mortality, future morbidity, and future medical care expenditures of cases that newly occurred in 1 year, say 1962.
3. A complete accounting of direct costs by disease category would be useful and is practical. A similar accounting of indirect cost would be interesting, is perhaps not so practical, and may not be equally meaningful.
4. What are some practical approaches to calculating the economic value of relief from pain, grief, etc? Empirical work is indicated.
5. One way to approach the issue of general vs. categorical support for research is to review and compare the quality of rejected applications for research support in various fields.
6. Economists need data on the effect of services on the health of a population, which they are not equipped to compile.

APPENDIX

Questions for Discussion

1. Is it meaningful to ask of economists, how much can this Nation afford to spend on medical research? If this is not a good formulation of the question, what would be a sensible way to ask, how much should we spend on medical research 10 or 15 years hence?

Background Issues:

a. What are the dimensions of the social welfare function, or what is our conception of it, as it relates to medical research expenditures?

b. If the Nation can afford more expenditures on medical research, why aren't these added expenditures being made? Lack of information? Why should these added expenditures be made?

c. In assessing the amounts we should spend on medical research, how do we evaluate the costs and returns of this activity against other types of research and development and against all other types of expenditures.

- Nature of the production function for medical research.
- Measurement of "'physical output" of medical research.
- Possibilities for successful exploitation of research findings.
- Evaluation of economic benefits (reduction in economic losses arising from the incidence of a disease) of medical research and associated health expenditures.
- Measurement of benefits, both direct and indirect.
- Timing of benefits.
- Choice of discount rate.

d. How can current evidence be used as a basis for allocating expenditures in the future—over the next 10 or 15 years?

2. Currently government (mostly Federal) pays for two-thirds of all medical research expenditures. Is it reasonable to project this figure into the future? In other words, are there any economic criteria for determining what government should pay for in medical research and what business and philanthropy might be expected to do? Moreover, within the public sector, is there a proper role for State and local government.

Background Issues:

a. In what sense is the present distribution of expenditures optimal?

b. What is the expected division of financial support between government and other sectors?

- In applied research we might expect a more-nearly "adequate" amount of support from the private sector but considerable underinvestment in basic research. For foundations the reverse is likely to be true but their financial resources are quite limited.

c. Thus, what is the rationale for government financial support of medical research?

- Uncertainty elements.
- Appropriability of benefits.
- "Public goods" aspect.
- External effects.
- Economies of scale.

d. Might there be reasons for or methods of stimulating the financing of more research by the private sector?

e. Is the role of State and local governments likely to be limited except where the regional incidence of a disease or affliction is high, e.g., air pollution in Los Angeles?

3. Are there criteria to help determine how much of government's medical research expenditures should be devoted to broad general research and how much should be devoted to special areas, such as heart disease, cancer and stroke?

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Public Health Service

Onward the Management of Science: The Wooldridge Report

NIH was not cleared on all counts by the Wooldridge Committee, which itself is rated low on methodology.

Joseph D. Cooper

After a year's survey of the research and management operations of the National Institutes of Health, a White House–appointed committee, chaired by Dean E. Wooldridge, reported to the President that (i) the scientific activities of the National Institutes of Health "are essentially sound" and that the public is getting its money's worth for the approximately $1 billion a year now spent through NIH; (ii) the *organization* and *procedures* of NIH must be changed in order to avert "substantial problems" in the future.

The Wooldridge report ["Biomedical Science and its Administration: A Study of the National Institutes of Health" (report to the President, February 1965)] was represented to be an effort to bring dispassionate, professional judgments to bear on questions pertaining to the future of NIH. The report is singularly free of the marketplace tone and stridencies of some reports to the public on health matters. In itself, this is a great gain. The substantive recommendations, however, while impressive, are likely to provoke spirited challenges. This, too, is wholesome, for progress is promoted by open, constructive dissent.

The substantive findings and recommendations of the Wooldridge Committee and its panels are discussed here, but they are, in a sense, of secondary importance. The larger question explored is whether the operation of the Wooldridge Committee itself should be taken as a suitable model for the conduct of future assessments of federal science operations. The President's Science Adviser and personnel of the Office of Science and Technology had hoped this would be one of the experiential outcomes of the Wooldridge Committee operation.

The creation of the Wooldridge Committee was one of the aftereffects of criticisms of NIH made by the Fountain Committee (the Intergovernmental Relations Subcommittee of the House Government Operations Committee, Representative L. H. Fountain of North Carolina, chairman). The Fountain Committee had charged that NIH had both committed and encouraged loose practice in grants management and that the university grantees had not assumed responsibility for prudent management of public funds. Although the Fountain Committee had not gone very deeply into appraisals of scientific quality, it had raised questions which some felt had to be resolved one way or the other.

Apart from this background, and its continuing implications, it had also become apparent that the time was at hand for a full-fledged review of NIH operations in the light of NIH's phenomenal growth. In the summer of 1963, therefore, President Kennedy directed the Office of Science and Technology to make the study discussed here, which got under way early in 1964 after the need for such a review had been reaffirmed by President Johnson.

Summary of Findings and Conclusions

In brief, the Wooldridge Committee made these findings and arrived at these conclusions:

1) The "large majority" of NIH intramural and extramural research is of high quality.

2) The large, centrally managed, collaborative research programs—the so-called "crash programs"—have not been well managed, have not produced gratifying results, and have not been well designed scientifically. The program of the Cancer Chemotherapy National Service Center came under particularly critical review.

3) The best available method for awarding research grants is through the study-section procedure, which utilizes scientific peer judgments.

4) The NIH needs urgently to strengthen its management capabilities, especially if it is to be able to assume growing responsibilities in directed research, demonstration projects, training programs, long-range planning, and the ensuring of optimum utilization of its funds.

5) Overall grants-management responsibilities of the universities must be stengthened. Where university action to strengthen grants management is not forthcoming, NIH should use its granting authority as leverage to "encourage" administrative improvement and to punish unresponsive universities.

6) The authority of the director of NIH should be strengthened while, at the same time, the autonomous status of categorical institutes should be diminished. There should be a strengthening of staff support for the NIH director, particularly in the area of program planning.

7) A new Policy and Planning Council should be created to assist the director of NIH in making of major plans and policies, especially those related to allocation of funds and resources. The new council should be encouraged by appropriations committees of the Congress to participate in the annual budget hearings.

8) The organizational stature of NIH should be upgraded by giving the director "easier access to and greater participation in the councils of higher authority in the Department of Health, Education, and Welfare," especially at the policy level of the Secretary.

9) The intramural program of NIH should be reviewed to assure that, for all activities, there is justification for conduct by the government. The report inferred that the desirability of government conduct of "independent, university-like research" is subject to question.

10) The present limitation of indirect project costs to an arbitrary

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percentage of direct costs should be replaced by a new system. All costs, direct and indirect, should be itemized and, if less than 100 percent is to be paid, the same fractional reimbursability should be applied to both direct and indirect costs.

11) The investigator should be made to feel that his salary comes from the university. Accordingly, the costs included in a grant which are attributable to the salary of the investigator should be separately negotiated between NIH and the institution.

Several other recommendations were made, including the following.

The director of NIH should be given greater discretion for the transfer of funds from one category to another. This would permit him to accommodate unanticipated program needs and to use unexpended program balances in some areas for the relief of shortages in others.

The amounts of grants for the support of general research should be enlarged in order to give university administrative authorities greater control over the content and direction of research programs. These grants should be made available for science departments outside the medical schools.

In recognition of the increasing quantification of the biological sciences, NIH should encourage greater participation of physical scientists and mathematicians in all aspects of its operations.

The administrative executives of grantee institutions should be given authority to make a variety of project-related decisions at present reserved for central NIH determination.

Organization of the Report

Before commenting on the specific findings and proposals of the Wooldridge report, I want to discuss its organization. The document as a whole is 213 pages long. The committee's official judgments are contained in the first 49 pages. All else is addendum material with which the committee said it may or may not have been in agreement. These supplementary materials are the separate reports of the panels and special groups set up to advise the Wooldridge Committee.

The overall committee, consisting of distinguished educators, industrialists, scientists, and philanthropists, devoted itself to issues cutting across the NIH effort as a whole. Advising the Wooldridge Committee were 11 panel groups of which eight were devoted to the life sciences: the anatomy, behavioral sciences, biochemistry, biophysics, microbiology, pathology, pharmacology, and physiology panels. These, it will be noted, ignored the categorical organization of NIH research. To complete the list, there were an administration panel, a physical sciences panel, a review procedures panel, and a group of *ad hoc* panel members.

When any reference is made here to the findings, conclusions, or recommendations of the Wooldridge Committee, it pertains to material within the first 49 pages. Likewise, a reference to the "report" refers only to material in pages 1–49.

Whether or not it was so intended, this format permitted statements to be made and published without responsibility for them being attributed to the Wooldridge Committee itself. Some very strong statements were made in this way, including one on cancer chemotherapy and collaborative programs in general, and another on congressional involvement in such programs. The committee recommended a careful reading of individual panel reports, which serve also as state-of-the-art reports and projections. It should be noted, however, that on topics dealt with in common, the panels were not necessarily in agreement.

Some mention should be made of the relationships between panel members and NIH. The eight life sciences panels had, in all, 74 members, of whom eight were government employees. Of the remaining 66, 58 were listed in the 1963 research grants index of NIH as project grantees. From all 11 of the advisory panels, 38 members were listed in the 1963 edition of *Roster of Members of PHS Public Advisory Groups* (Public Health Service publication 262A). One member of the committee itself also was listed in the *Roster*. These project grantees and PHS advisers were asked to make assessments of the output of a system of project approval in which they were intimately involved both as grantees and as members of peer review groups. The defense given for this aspect of the system is the usual one: you simply cannot find many competent scientists who are not under NIH grant support and not members of study sections and other advisory groups. This is undoubtedly so. Still, it does not dispose of the subtle influence on judgmental processes which occurs when people who are part of a system are asked to review the products of that system. The report did not indicate that many panel members and advisers were either grantees of NIH or members of NIH or PHS advisory or review groups.

Notably absent from membership in the Wooldridge Committee itself or its advisory panels were prominent medical research men and others who have been influential in multiple roles as "citizen witnesses" before appropriations committees, as members of NIH advisory councils, and as participants in a number of national advisory bodies. This group includes such people as Michael DeBakey, Sidney Farber, Philip Handler, Isador Ravdin, and Howard Rusk, among others, who have carried a heavy load in these roles.

One explanation is that most of these people were already members of the President's Commission on Heart Disease, Cancer, and Stroke—a commission whose life span largely paralleled that of the Wooldridge Committee. In fact, Philip Handler of Duke University was originally to have been chairman of the biochemistry panel of the Wooldridge Committee but resigned when appointed to the President's Commission.

Is NIH a Science Agency or a Health Agency?

The Wooldridge Committee asserted that "NIH devotes its principal efforts to a broad program of investigation of life processes, rather than to a search for direct cure or prevention of specific diseases." The committee stressed the desirability of such a "normal science" approach and attacked the "head-on" approach as being "frequently the slowest and most expensive path to the cure and prevention of disease." Note that the Wooldridge Committee didn't merely state that this is how NIH should conduct its work. Rather, it asserted that this is the way things actually are. This fact, it observed, may not be generally understood. "Cursory reading of the various legislative acts that establish NIH powers and responsibilities would not make this point clear," stated the report. "Similarly, the employment of such labels as 'heart,' 'cancer,' 'neu-

rological diseases and blindness,' and the like, in the titles of the major organizational units of NIH, suggests more of an orientation to specific diseases than actually exists. Although there are practical reasons for such an overemphasis on disease categories in the organization structure, it is not ideally suited to the kind of research programs which must actually be carried out. One of the accomplishments of the NIH management has been that of making a scientifically inappropriate organizational structure an effective arrangement for performing its real mission."

In short, the report states that NIH is not a disease-oriented organization. It is, rather, engaged in the support of fundamental research into life processes along normal disciplinary lines. While NIH justifies its programs to the Congress and to the public in terms of drives on various disease fronts, these are merely "practical" expedients through which NIH has to operate, stated the Wooldridge Committee.

A notable deficiency in this argument is any discussion of the overall mission of NIH. It would seem essential to go into this matter in some depth before making recommendations affecting mission. The Wooldridge Committee and its panels do present a concept of mission, which is for NIH to function mainly as an instrumentality for funding biomedical science organized along disciplinary lines. What seems to have been overlooked is that NIH is also a public instrumentality. Its growth up to its present dimension was made possible because forces of persuasion convinced the public that, through NIH and its categorical institutes, massive assaults would be launched against various disease entities. Whether or not this is best from the standpoint of normal science is an argument apart. There is still the serious matter of accommodating the expectations of the public. Besides, a persuasive case might also be made for disease-oriented research as having its own fundamental outputs. The classic example in this regard is the development of the poliomyelitis vaccine, a project of the National Foundation. From the standpoint of the public this was a successful outcome in a campaign against a specific disease entity. As viewed from a broader scientific standpoint this mission stimulated advances in virology which continue to have their referred and deferred effects, not the least of them being in connection with the possible viral etiology of cancer. There are, thus, many roads to Rome. The NIH travels many of them. It needs to do so in the future, to assure that progress will be balanced and rapid.

A related problem, hinted at tolerantly by the Wooldridge report, is the public posture of NIH in telling what it is doing. Is it proper to assume the role of disease chaser in the popular prints while playing the more correct role of fundamental researcher in scientific circles?

Assessment of Research Quality

The report's general endorsement of the quality of research "currently supported" by NIH seems to raise more questions than it purports to answer. There is, first, the overall question of the relationship between good management and research quality. Here we have a dilemma: if the quality of management has been deficient both within NIH and in the universities and if, nevertheless, research continues to be of high quality, why be concerned about good management other than to make sure that no funds are improperly diverted? On the other hand, it might be more realistic to ask whether there *can* be a consistently high level of research quality in the face of inadequate management.

Then there is the matter of the rate of growth of NIH appropriations as compared to the expansion of the scientific community dependent upon NIH. "Despite the tenfold increase in NIH support of research during the last eight years, there is no evidence of over-all degradation in quality of the work supported," stated the report. "On the contrary, there is good evidence that the average quality is steadily improving. This appears to be a consequence of a rate of increase in the production of competent biological research scientists matching the over-all rate of increase in research support." For this, the report offers no documentation. It takes from 10 to 12 years to double the scientific population. One must assume that in this doubling there is no more than a proportionate increase in numbers of scientists of excellence. One might well infer, to the contrary, that exponential increases in funding attracted marginal and submarginal investigators in some degree. This could be inferred from the fact that observations made just before the major period of NIH growth, which began in 1957, indicated that the supply of first-rate researchers was then fully committed. The Wooldridge Committee, in effect, concluded that a politically determined level of spending brought into being a corresponding number of competent scientists.

In raising these challenges I do not charge that NIH research, whether intramural or extramural, is not of high quality. I have no way of knowing. It does not seem, however, that the Wooldridge Committee satisfactorily disposed of the question.

The committee said that it had confidence in the validity of its conclusions about quality because of the competence and distinction of its consultants and the extensiveness of their survey. The committee went to considerable pains to explain the methodology of the survey, especially the statistical sampling method used with the advice and assistance of a panel of three distinguished statisticians. The committee did not, however, go into the question of whether the members of its advisory panels should have been asked to evaluate the products of a system of which they were a part. Nor did the committee reveal the criteria followed by panel members in reviewing research projects to determine their quality. It is understood that such criteria were given the various panel members, but part of the understanding with the institutions which were to be visited was that all project reviews and even the criteria pertaining to them were to be held confidential. It is an interesting point, for it raises a collateral question of public access to information about how public funds are spent.

The report stated: "Out of the 240 'traditional' extramural research grants investigated, the panel teams expressed serious reservations about 9 projects and adjudged an additional 7 to be unworthy of support. In scientific research, such a ratio of ill-advised projects, when judged after the fact, is impressively low. Much more frequently, NIH-supported work was found to set the national or international standard of excellence in its field."

One would hope that the statistical sampling was proper and that subjective factors in evaluation were not

at work, for these figures are impressive. There remain the questions raised above. Also, the report did not include any comment on 268 other, nontraditional research grants also investigated. Neither did it draw attention to this selective subsampling.

If the findings on quality are to be believed, one must reevaluate the many, many criticisms of recent years to the effect that the scientific literature (the products of research) is bogged down by an abundance of trivia—bibliographical buildups, replications of work already done, and data of questionable value. While such allegations are subjective judgments, the fact that they are made by responsible individuals leads one to conclude that there must be something to them. Perhaps this is an area for a new committee to investigate.

The assurance of quality is further clouded by some knowledge of the realities of grantsmanship. It is fairly common knowledge that the right labels are often placed on the wrong bottles. Research projects may be designed to support a teaching position or a principal investigator who spends much or most of his time away from the university, discharging the duties of a "research entrepreneur" rather than those of a conductor of research. Moreover, it is a fact that the timing of a new research project is more likely to coincide with the need for maintaining a steady or rising state of staff support than with bursts of creativity. In themselves, none of these comments refutes the major finding on quality of the Wooldridge Committee. Rather, these questions are raised as gross challenges because the scientific community is left in the dark as to the nature of the criteria of review.

On a related matter, the Wooldridge Committee endorsed wholeheartedly the peer-judgment mechanisms of approving requests for research awards. The committee and its review-procedures panel were almost lavish in their praise of the panel system. The question raised here, however, is whether the Wooldridge Committee addressed itself to the pertinent issue—that of whether the panel mechanisms can be strengthened, not of whether they are necessary.

The stream of criticism which has been directed in recent years against the panel mechanism by Alvin Weinberg and others has been to the effect that panels, for all their virtues, tend, through cults of personality, to inhibit progress. The Elliott Committee pointed to the growth of a "panel establishment." The consensus of past constructive criticism has been that panels could stand more membership rotation than they actually have had. The Wooldridge Committee did not, however, deal with this problem. Moreover, scant attention was given the role of advisory councils in project review. There was a time when advisory councils were able to review projects individually because there were fewer projects than there are now. In fact, during the earlier days of NIH growth, descriptions of new projects were published in the appropriations hearings. Now, advisory councils rarely examine individual project proposals.

One other study which dealt with the panel situation was that of the Kistiakowsky report [National Academy of Sciences, Committee on Science and Public Policy, *Federal Support of Basic Research in Institutions of Higher Learning* (1964), George Kistiakowsky, chairman]. The request for this study was made in a resolution of the American Society of Biological Chemists and other scientific groups as a direct response to the activities of the Fountain Committee and the actions it had obliged NIH to take. The principal contribution of the Kistiakowsky report was to call for strengthening of weaknesses in the panel system through which research proposals are evaluated and to call for new types of research support to correct imbalances.

Criticisms of Collaborative Research

The criticisms of the quality of collaborative research must be considered within the framework of the larger thrust of the Wooldridge Committee and its panels. This larger objective appears to be that of bringing more firmly under the outside academic establishment the control of the federal effort in biomedical science—that is, to make this effort conform to the academic disciplines. As mentioned earlier, the committee insisted that the most rapid progress against specific disease entities is likely to come not from organization by disease categories or from crash programs but from continuing inquiry into the life processes. Hence, it is understandable that all the recommendations of the Wooldridge Committee on the conduct of science are oriented toward the values of the academic establishment.

On the other hand, the committee's criticisms of science programs are concentrated on the products of what NIH staffers call The Establishment, consisting of a mutually reinforcing group of "citizen witnesses," key members of congressional appropriations committees, key officials in the executive branch, and the leadership of the Mary Lasker organization (a philanthropic group which concentrates on expansion of federal health programs). The citizen witnesses are those who testify before the appropriations committees; who dominate the advisory councils and the occasional commissions on growth of the enterprise; and who were responsible for the inception of at least several of the crash collaborative research programs, in particular the cancer chemotherapy program.

One can undoubtedly find much support in the scientific community for criticism of the collaborative research programs. "NIH is experiencing difficulty in achieving the results desired from its large collaborative activities," stated the report. "In particular, many medical scientists question whether the value of the Cancer Chemotherapy Program, which accounts for more than half of all Collaborative Program funds of NIH is commensurate with its cost. We advocate the appointment of an *ad hoc* committee to make a study of this program and to recommend any necessary changes in direction."

A subpanel of the Wooldridge Committee said of the Cancer Chemotherapy National Service Center: "Because the availability of money exceeded the availability of sound ideas, some unsatisfactory effects inevitably resulted. . . . We believe . . . that the CCNSC should have been taken through an extensive pilot project stage before its full-scale launching." This is not a new idea; opponents of the mass trial approach took the same position when the cancer chemotherapy program was first being debated. They were, however, overruled because of continued pressures by Establishment members.

The committee noted that the current budget for cancer chemotherapy is approximately $34 million a year but that related projects bring the total to approximately $47.5 million a year. Even at the lower figure, the commit-

tee noted, this is the lion's share of all NIH collaborative research, which in fiscal year 1964 was $58 million. The suggestion was that some of the money spent on cancer chemotherapy might more constructively have gone for other pursuits.

Although the committee did not make specific critiques of other collaborative research programs, it identified several of them. The Psychopharmacology Service Center received no mention at all. It was explained that this program had not been picked up by the sampling procedure. This is regrettable since the psychopharmacology program has been so vigorously touted by certain Establishment members as to command a great deal of public interest. A review of this program would reveal difficulties of a different kind from those found in the cancer chemotherapy program. In some respects the problems are even more critical, for they involve the quality of research on human subjects. One wonders, also, why the collaborative project on perinatal disease received no comment, since it is open knowledge that it is in trouble.

The Wooldridge Committee criticized the National Cancer Institute in particular for having dropped outside advisory consultants from the cancer chemotherapy program. The NCI said it did so because it found that some of its professor-consultants had too many conflicts of interest. That is, in addition to being professors they were consultants to drug companies, often had proprietary interests in drug enterprises, and were advising the National Cancer Institute on the awarding of contracts to enterprises in which they had interests. The Wooldridge Committee felt, however, that NCI took advantage of this situation in order to divest itself of outside consultants. The committee felt that NCI could have found acceptable advisory panel members.

The committee reaffirmed the desirability of having collaborative programs as a function of NIH but listed certain prerequisite conditions.

1) Use of outside experts to advise on the feasibility of proposed collaborative programs.

2) Exercise of an advisory review by the proposed Policy and Planning Council for all major collaborative programs.

3) Contracting flexibility to permit the NIH to turn over program-management responsibility for collaborative programs to outside organizations, when justified.

4) A policy of keeping each collaborative program as small as possible.

5) An inviolable rule that no large collaborative program will be started until a strong management team is available, regardless of how scientifically promising the program may be.

6) More dependence on continuing appraisal and advice by outside scientific consultants than has recently been the case.

There is a suggestion to the Congress that it desist in future from directing the setting up of various crash programs. While the committee itself avoided direct comment on this, one of its subpanels said: "The Congress should not be disenchanted of biomedical science by the seeming slowness of progress toward specific goals."

The subpanel then suggested that when the Congress identifies health priorities it should defer to the judgments of outside peer groups. Some criticism of both crash research programs and the categorical-disease approach to biomedical research is implicit in the panel's call for "a catholic attitude to recognize that discoveries made in the pursuit of one health problem can often be exploited to yield answers for a second health problem even before the first one has been solved."

Strengthening University Management of Science

The criticisms of the management of science programs both in NIH itself and in its grantee institutions come within the realm of normal expectation. A scarcity of managerial maturity seems to have become a concomitant of progress in practically all areas of science and technology as well as in the professions generally. One can even say that the professional manager who looks to the problems of others is often himself in need of repair, like the preacher and the cobbler. While this is a rationalization, it is not a justification for not doing something about problems once they have become identified. Moreover, in the NIH situation the problems have been aggravated by an unusually steep curve of growth.

The solution proposed by the Wooldridge Committee to enforce the upgrading of grants management in the universities is for NIH to use its grant-making leverage to compel good practice. "In the absence of adequate administrative improvement, NIH should substantially curtail the amount of its support for the institutional investigators," said the report. One can think of nothing that is more likely to raise the academic hackles than for NIH to say to a university: "Either you straighten out and fly right or we'll cut your grant support levels." After all the charging and countercharging that came in the wake of the Fountain Committee report, to the effect that it was urging interference with academic freedoms, the Wooldridge Committee proposal for direct federal intervention comes as something of a shocker. In fact, the classical argument against the federal grant-in-aid approach is this: first you woo them with money; then you get them to like its feel; and then you begin to impose conditions for their continued receipt of your money. By that time you begin to own them mind and body.

Centralizing Authority in NIH

The proposals to centralize authority in the director of NIH and to provide him with staff facilities for enhancing his program-planning capabilities should be considered in the light of previous patterns of NIH organizational growth. Most of the pressure for NIH expansion came from outside. The creation of both the Cancer Institute and the Mental Health Institute, the first two categorical institutes established within the overall hegemony of NIH, came about through outside pressures. Thereafter, other categorical institutes were added, each with its own outside supporters and beneficiaries. In the same way, collaborative research programs were superimposed on this structure, all without regard to any grand design which would assure balance and integration of effort. Each of the categorical institutes has its own statutory authority, its own appropriations, its own advisory council *responsible to the Surgeon General*, and its own group of citizen witnesses.

A natural fear on the part of the scientific community is that centralization of planning and authority in the hands of the director of NIH could lead to a monolithic, authoritarian

bureaucracy. Just as academicians in the universities resist the imposition of authority by their own administrative officers, so NIH scientists in the categorical institutes would be likely to object to an authoritarian structure. In this, they might well be supported by their outside academic colleagues, who could be expected to say, "Though the present leadership consists of 'good guys,' who might succeed them in the future?"

Certainly the Wooldridge Committee did not have in mind any such developments. Rather, it sought a redress of balances to enable the NIH director to exercise an overall responsibility for programs where, before, his resources for such purposes were extremely limited. Nevertheless, the proposed concentration of authority is a change in the power structuring of NIH which would have to be brought off with consummate skill in order to preserve the advantages of decentralization, fragmentation, and scientific pluralism within the framework of overall planning.

A corollary proposal is to create a Policy and Planning Council which would advise the NIH director, review major new program proposals (especially proposals for collaborative research programs) and, if the Congress is willing, provide budgetary advice. Of course, the proposal has merit, for there is now no overall advisory council to which the NIH director can turn for guidance. There is, however, the risk that a new elite group could thereby be created which might dominate a less independent personality than the present director, James A. Shannon. This, of course, is no reason for not embracing the proposal. Rather, the argument comes under the heading of customary caution.

The Wooldridge Committee made a point in advancing the principle of providing independent "advice" (rather than governance) to the NIH director. It went further in suggesting a specific mechanism, and in urging its expeditious creation. It might also have suggested viable alternatives, but it did not do so.

What is likely to provoke more than merely spirited reaction behind the scenes is the suggestion that Congress turn to a new source of scientific guidance in the development of its own budget judgments pertaining to the NIH. As the proposed Policy and Planning Council would consist of experienced and distinguished scientists in addition to a minority of outstanding nonscientists, one possibility is that its members could supersede the individuals who comprise the so-called "professional judgment budgets" upon which the House Appropriations Subcommittee relies. As a source of judgments its members could supersede the citizen witnesses upon whom the Senate Appropriations Subcommittee relies. One cannot imagine that a shift in the locus of exogenous power would be effected very peacefully. At the very least one might expect a lively competition for membership on the new council, if it should be established and if it should bring about a shift in locus of informal elite power.

Organization of NIH

The proposal to give the director of NIH greater access to the highest councils of the Department of Health, Education, and Welfare was left vague, presumably because of ongoing studies of HEW-PHS-NIH structure. On the one hand there seems little doubt that the phenomenal growth of NIH within the framework of the Public Health Service has created organizational problems. It is probable that, as the scope of health programming in the federal government broadens further, many separate agencies will become operationally involved. This lends support, perhaps, to current proposals to establish a Department of Health. On the other hand there are arguments for bringing NIH, the Office of Education, and the National Science Foundation organizationally closer together, because of their common interest in building up the academic establishment.

One related question not touched upon at all is the distinction between functions of NIH and of the National Science Foundation. The original concept for NSF was that it would be a source for the funding of free, nonmission-oriented research, including the broad area of biomedical science. The overall thrust of the Wooldridge Committee would seem to be in the direction of expanding what should be an NSF capability, as stated in the NSF charter, through the structure of NIH.

This may be the time to consider other holdover issues. For example, should the Division of Biologics Standards remain in NIH or should it be combined with the Food and Drug Administration, with which it should have greater homogeneity of interest? The National Institute of Mental Health is becoming extensively involved in the conduct of community action programs; this suggests the possibility that it has outgrown NIH. Unlike the other categorical institutes, it is heavily involved in the politics of federal-state relations. One possibility might be to retain within NIH, as a research organization, only those activities which are of an academic-research nature, relating to the biological bases of mental disease, psychopharmacology, behavioral phenomena, and related work.

The suggestion that NIH might be divested of at least some of its intramural activities calls for the most careful consideration. The Wooldridge Committee said that it "is not convinced that it is sound for the Federal Government to conduct large amounts of scientific research of the kind that non-Federal institutions are equipped to carry out," and that "a majority of the NIH intramural program appears to be of this nature." The NIH intramural scientist is described as having at least as much "academic" freedom as his university counterpart, and probably more. Furthermore, he is described as having fewer "red-tape" annoyances and fewer distractions such as faculty meetings, department administrative assignments, committee activities, and the like. "Not being in an educational institution, he need not teach; he can devote all his time to research." The lack of teaching responsibility as a possible partial basis for the development of the large intramural establishment was examined by the Wooldridge Committee. "Nonteaching positions in basic health research are certainly not common, and perhaps there is here a legitimate hole for the government to fill in the nation's health science structure. However, we suspect that the size of this hole may not be very great."

The most obvious comment one could make is that nonteaching positions on a *de facto* basis are plentiful throughout the academic community. Why the lack of teaching responsibility at NIH should be a matter either for implied criticism or for envy is difficult to understand, unless it is that NIH intramural researchers don't even have to pretend they are teaching. As for NIH scientists' being relatively free of administrative and bureaucratic

distractions, one has but to try to get some of them on the telephone. He will discover how universally their time is consumed in attending meetings, fulfilling administrative requirements, and so on and on.

If anything, NIH scientists are to be commended for their concentrated adherence to purpose in the face of financial disadvantage. In contrast, their academic colleagues, in many or most cases, can avail themselves of consulting and summer research opportunities which bring them substantial incomes in addition to their base salaries, which already exceed those paid by the government to their scientists, the latter situation being a historic reversal. Ironically, it is NIH fund-granting which has helped bring this about.

The only action on the intramural issue called for at this time by the Wooldridge Committee is a study by the proposed Policy and Planning Council, although the report goes quite far in intimating what might be the outcome of such a study. What the committee did not bring out is the fact that the intramural program has escaped criticism to a singular degree. It is one of the bright spots in the NIH firmament. The committee suggests that the intramural staff members have little impact on the extramural programs, and that such an impact cannot be used as an argument for maintaining intramural activities at present levels. If this should actually be the case (the committee did not document this statement) it might be desirable to enhance the intramural scientist's role as reviewer and consultant in grant-making.

One can see other reasons for having a strong intramural capability in considerable depth. Thus, the national interest might well be served by having a series of independent research units which are free, as the intramural laboratories are, of the preoccupations and maneuverings associated with "grant-swinging." Furthermore, the Wooldridge Committee has not made a case *against* the idea of having an independent type of research organization, with no teaching obligations, nor has it made a case for a virtual monopoly of research by the universities. One could, in fact, muster arguments in behalf of having more independent research organizations dissociated from the universities. Admittedly, these considerations were outside the frame of reference of the Wooldridge report, but they bear on the intramural issue.

The "enrichment value" of a strong intramural program seems not to have been considered. This is a value the committee could not readily identify or weigh. Nevertheless, it would seem logical to argue that a strong intramural program, which includes advanced work by distinguished scientists, helps in recruiting and retaining the total NIH staff, both intramural and extramural.

In a pluralistic system, such as science should be, there is some value, also, in having as many independent points of view as possible. If NIH is to build up its overall planning capability it will need to have contributions from both its intramural and its extramural staffs.

Payments to Universities

The indirect-cost issue has been festering its way through a seemingly endless series of investigations and reports by the scientific establishment, the Congress, and the executive branch. The Wooldridge Committee, in a new approach, now argues that it is unfair and unrealistic to distinguish between direct and indirect costs. In other words, all incoming work adds to the burden of overhead, which then must be funded in some way, usually through diversion from other university programs. The committee suggested that ordinary industrial accounting practices be followed. All overhead costs would be allocated to direct costs in arriving at total costs to be charged to the customer. If the customer—the federal government—then wants to pay less than 100 percent of total costs it should do so across the board, not distinguishing between direct and indirect costs. Presumably the university would then be able to budget the aggregate load of research it could support if it must make a matching contribution.

The proposal that salaries of investigators not be included in the research grant proposal deserves careful study. The reasoning was dual: the investigator should not be demeaned by having to negotiate his own salary with his professional peers; his loyalty to his own institution would be strengthened by having his salary more dependent on the negotiations of the latter with the NIH. While this proposal may be in the right direction, the niceties would need careful engineering. It would seem that salaries within a university need to be considered within *its* total pattern.

Conclusions

Progress is being made, undoubtedly, in the development of mechanisms for assessing federal science operations. One must conclude, however, that the Wooldridge Committee approach does not serve as a model for the future. The committee did not avoid built-in biases of interested parties in selecting its advisory panels. It did not publish the criteria it used in making assessments of research performance. It did not propose alternative actions. It did not provide adequate documentation either to support its own findings and recommendations or to enable readers to make independent assessments.

Finally, any study of NIH can be little more than a fleeting view of a moving scene. New program dimensions generate new relationships and, in turn, new stresses and strains. Minor difficulties become major ones. Old problems fall out. The full implications of the proposed regional medical complexes, recommended by the President's Commission on Heart Disease, Cancer, and Stroke, are yet to manifest themselves. One of the issues engendered by this proposal is, Who shall administer the new research funding for the medical complexes? Whether or not this is to be done through NIH is yet to be decided. (At the time of writing, the HEW conclusion was that NIH should have this responsibility.) In any event, new patterns of funding academic research are likely to emerge—together with new problems.

CRITERIA FOR SCIENTIFIC CHOICE II: THE TWO CULTURES

ALVIN M. WEINBERG

The Financial Support of "Science as a Whole"

IN a previous paper [1] I proposed criteria which could be invoked in judging how to allocate support to different, competing branches of basic science. Such allocations seem to be necessary because what society is willing to spend on all of science is not enough to satisfy every worthy claim on the total funds available for science. I turn now to the broader question: what criteria can society use in deciding how much it can allocate to science as a whole rather than to competing activities such as education, social security, foreign aid and the like?

That such a question can assume any urgency is in itself remarkable. To have suggested that the Federal Government of the United States would be spending about 3 per cent. of the gross national product for research and development would have been unbelievable 25 years ago. Most of the new attitude toward government support of science and technology was prompted by war and fear of war. In the mind of the public, scientific strength has been equated with military strength. Support of science at first was only dimly distinguished from support of the military. But this attitude is changing, partly because the thermonuclear stalemate seems to have reduced our fear of war, partly because the fantastic successes of modern science have begun to penetrate the awareness of the public. Science *per se*, as a valid human activity supported by the public, has acquired some standing, possibly analogous to that of religion in the era before the separation of church and state. As science has become big, it has acquired imperatives, just like any other activity of government, to expand and to demand an increasing share of public resources, and now, for the first time, it has become big enough to compete seriously for money with other major activities of government.

The criteria for choice between different fields of basic science I proposed earlier were of two kinds—internal and external. Internal criteria could be established entirely within the scientific field being considered; these criteria arise from the question: how competently is this field of science performed? External criteria could be established only from outside and

1 "Criteria for Scientific Choice", *Minerva*, I (Winter, 1963), 2, pp. 159–171.

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answered the questions: does this field of science illuminate other fields of science; does it further desirable technological goals; does it further broad social goals? My main point was that a good rating according to the internal criteria was a necessary but not sufficient condition for large-scale public support of a field of science. Only if a field rated highly according to criteria generated outside its own universe could it properly expect large-scale support by society.

In so far as the support of science as a whole can be viewed as different from support of each of the separate branches and kinds of science, I believe one can apply analogous criteria. Society, in its support of science, assumes that science is a competent, responsible undertaking. But society is justified in asking more than this of "science as a whole". However vaguely stated, society expects science somehow to serve certain social goals outside science itself. It applies criteria from without science—broadly, criteria concerned with human values—when it assesses the proper role of "science as a whole" relative to other activities. We scientists concede this implicitly when we agree that responsibility for choosing between science and other activities belongs primarily to the non-scientists —the members of legislative bodies or the head of the executive branch of government and his staff. In the language of Stephen Toulmin [2] the choice between "science done for its own sake" and other activities of the society is a political choice, as contrasted to an administrative choice, and it is to be made by politicians.

The ordering of human values upon which such choices must ultimately be based is a philosophical question into which I will not enter here. I shall assume that we have decided on social goals and shall then ask how we can translate these into practical recipes for deciding how much science we can afford.

The Budgetary Separation of Pure and Applied Science

I shall dispose of the question of what fraction of society's overall effort should go into "science as a whole" by arguing, along with many others, that "science as a whole" is a misleading idea. The basis for the claim which applied science makes on society is so different from that of pure science that lumping them together clouds the issue. Pure and applied science ought not to be viewed as competing for money.

Applied science is done to achieve certain ends which usually lie outside of science. When we decide how much we should allocate to a project in applied science, we at least implicitly assess whether we can achieve the

[2] "The Complexity of Scientific Choice: A Stocktaking", *Minerva*, II (Spring, 1964), 3, pp. 343–359.

particular end better by scientific research than by some other means. For example, suppose we wish to control the growth of population in India and suppose we have at our disposal $\$200 \times 10^6$ per year for this purpose. We could devote most of this sum to investigating fertility, to developing better contraceptive techniques, or to studying relevant social structures in some Indian village. Or, alternatively, we could use the money to buy and distribute existing contraceptive equipment, such as Gräffenberg rings, perhaps using some of the money as incentive payment to induce women to accept the technique. Which way we spend our money is a matter of tactics; evidently no general proposition can tell us how much of our effort ought to be spent on research rather than on practice in trying to achieve effective birth control in India. The scientific work that goes toward solving this problem ought to compete for money with alternative, non-scientific means of controlling the growth of population in India rather than with the study of, say, the genetic code. More generally, where a piece of research is done to further an end which society has identified as desirable, support for this type of scientific work should be considered as part of the bill for achieving the end, not as part of the "science budget". Only that scientific research which is pursued to further an end arising or lying within science itself should be included in our "science budget".

This view has become quite popular in many recent discussions of the subject.[3] It is appealing to the scientist because setting support for applied science outside the science budget reduces the latter enormously—from $\$16 \times 10^9$ to perhaps $\$1 \times 10^9$. At this level the whole question of choice between scientific and non-scientific activities becomes much less significant.

But this stratagem is not as clearly justified as it appears at first sight. Ruling applied science to be part of the budget of non-scientific activities, not of the scientific budget, does not eliminate competition between applied science and basic science. Applied science requires at a secondary level, by and large, the same kind of people as does basic science. Building a large accelerator engages electrical engineers who would otherwise be available to help design control systems for rockets. In allocating support for a given applied science, one must keep in mind the effect of such allocation on basic science, and in supporting basic science, one must keep in mind the effect on applied science. Edward Teller has argued that because of the great emphasis on basic sciences in our universities, we have created an atmosphere that is uncongenial to applied science. He insists that our important applied scientific undertakings suffer because we tend to direct our best talents to basic science, our not-quite best to applied

[3] A particularly cogent presentation of this position is made by Stephen Toulmin, *op. cit.*

science. Though Teller's contention is difficult to prove, my own experience supports his view.

A second difficulty is that the aim of any given branch of applied science tends to become diffuse as time goes on. The scientific work of any of the large "mission-oriented" government agencies started out specifically to further the mission of the agency. But as time has passed, these clearly defined, "mission-oriented" goals of applied scientific work have become fuzzy. Byways that originally were germane to the mission flourish—an investigation that began as a promising approach to solve an applied problem, 10 years later becomes an interesting study pursued for its own sake, yet it continues to be described as "applied science". Thus to leave applied science out of the science budget would leave out a large amount of research which was at one time motivated by an extra-scientific or applied end, but which is now pursued primarily because it is scientifically interesting to those carrying on the research.

Finally, the motivation for basic science is itself often less than pure. Is nuclear structure physics done to further science or to help build reactors? Is the structure of natural products pursued as a challenge to scientific virtuosity in organic chemistry or because out of such studies will come the knowledge of enzyme action which ultimately will lead to control of metabolic disorders? Thus consideration of support of basic research completely apart from applied research is not as clearly defined a proposition as many proponents of this position hold.

Nevertheless, I believe the *general* principle of not considering the budget for applied research as part of our "national science budget" and including only basic research in it has one overriding advantage. By allocating funds to any applied research as a certain fraction of the budget of the (usually non-scientific) activity to which the research is intended to contribute, we keep straight our reasons for supporting the applied research. What this fraction should be must depend on internal criteria—such as, do we see ways of making progress, or are good research workers available? It probably should also depend on the impact that support of that field will have on neighbouring basic fields.

The fraction of effort that goes into achievement of a broad end—like aid to underdeveloped countries or national defence—by scientific research instead of by non-scientific action can hardly be decided entirely by the scientists. The scientific approach to solutions of difficult social problems is becoming increasingly popular. Yet in at least some proposals for action of which I am aware—notably in foreign aid and control of world population—it seems to me that excessive claims were made for science. Scientists alone, when asked to judge how to solve a complex social

problem, more often than not recommend more science—just as high-energy physicists, when asked to recommend a programme in basic science, will ask for more high-energy physicists or oceanographers for more oceanography. To overstate the capacities of scientific research as a technique for settling difficult social questions is no more sensible than it is to understate them. Thus, just as I have argued that scientific panels, judging how much money should be allocated to one branch of science rather than to another, should include representatives of neighbouring branches of science, so a panel determining how much scientific research rather than "engineering" or "production" will best achieve a certain non-scientific end should include non-scientists as well as scientists.

Support for Basic Science as a Branch of High Culture

I have argued in the foregoing that applied and basic science should have separate budgets and that the budget for applied science should be set as a certain fraction of the effort allocated to the end (usually non-scientific) which applied science furthers. To this extent I have avoided the problem of choice between "science as a whole" and other human activities by denying the usefulness of the concept "science as a whole". This still leaves the question of basic science—the science which cannot be justified by any reason except that it satisfies human curiosity. Are there some broad social ends, outside of basic science, which basic science serves, and to which its budget can be tied?

Obviously, some parts of basic science are important to applied science: in my view a much larger fraction of basic science is germane to applied science than many of my basic scientific colleagues are willing to concede. The bulk of the biological sciences is, in a sense, applied. For example, the most recondite and ingenious elucidation of the genetic map of *E. coli* is germane to the whole question of genetic abnormalities. (I often find it amusing to argue with my biologist friends that most of what they do is applied research—that the important distinction in a field of science intrinsically so close to human affairs as is biology is not between "applied" and "basic" but between "intelligent" and "unintelligent".) Or again, plasma physics, a purely basic science, is central to thermonuclear research, an applied science which is pursued because we wish to enlarge mankind's energy resources.

It is natural to propose that such basic research receive a certain fraction of the resources going into the applied research which it underlies. Every good applied research laboratory allocates to basic research a certain fraction of the resources allocated to it for its related applied research. The ratio of basic to applied research often is very high and is usually highest in

the applied laboratories which have had the most success in accomplishing their technological mission. What I suggest is that on the national scale, also, basic research be considered as a fixed charge on the applied research effort, wherever the basic research is intended to contribute to a field of applied science. In making an assessment of relevance, I would incline toward a broad interpretation: for example, I would consider the case of most research in biology as a proper overhead charge to be assessed against the resources allocated to agricultural and medical research.

But what about those fields of basic research, a few of them very expensive, which are really very remote from any applied scientific problems, which are pursued primarily because the researchers find the science intensely interesting, often because the findings in this field are likely to illuminate neighbouring branches of basic science? To what can we tie the allocation of effort for such activities?

This is the most puzzling of all the questions concerning public support of science and any proposed solution must be put forward most tentatively. For basic science of this kind is primarily a somewhat disinterested intellectual activity, in the same sense as are music, literature and art. Indeed, the analogy between the creative arts and this purest kind of basic science is sufficiently great to suggest that, insofar as it must make the choice, society might choose between the pure basic sciences on the one hand and the creative arts on the other. In allocating support for the purest basic research, our allocations for the other creative activities of man might be taken as our guide.

There are many analogies between the purest basic research activity and artistic activity. Each is an intensely individual experience the effect of which transcends itself. The product of each is immortal—the theory of relativity, just as surely as *Hamlet* or the Mona Lisa. Each is concerned with truth—the highest of human manifestations—the one with scientific truth (which deals with the regularities in human experience), the other with artistic truth (which deals with the individuality of human experience).[4] Each enriches our life in unmeasurable though highly significant ways. Each belongs not only to its creator or discoverer, but to all mankind.

In a competition for support between pure science and the arts, I see two major arguments—one that supports the claim of science and the other, the claim of art. The argument that favours science (aside from the obvious one, to which I shall return, that even the remotest pure science may eventually have practical application) is that scientific truth, being based on what we observe in nature, is publicly verifiable, whereas artistic truth,

[4] This point was illuminated for me in Barzun, Jacques, *Science: The Glorious Entertainment* (New York: Harper & Row, 1964), p. 227 *et seq.*

not subject to the same kind of control, is not publicly verifiable. Artistic critics disagree just as often as they agree. They have no objective and impartial arbiter, nature, to say what is true and what is not true. The truth of science, on the other hand, is rigorously and publicly tested by experiment or by observation or, in the case of mathematics, by logic. Scientific criticism weeds out scientific nonsense more efficiently than artistic criticism weeds out artistic nonsense because, ultimately, science is monitored by a universal and approachable critic, nature, whereas art has no comparable critic. Scientific research and thought, in their mutual and ruthless criticism which reach ever more strongly towards a whole consistent structure, are embedded in what Michael Polanyi has called the "Republic of Science"[5]—the entire scientific community whose mutual interaction is governed by rules of scientific conduct that are themselves laid down by nature, the great scientific lawgiver. The republic of science forces science to be a responsible undertaking, at least in the sense that what science does is true and, in some approximation, true forever. The corresponding republic of the arts has no such final arbiter that can force art to be as responsible as science. In so far as public support ought to go for the more responsible undertaking, the purest science in this regard merits more support than do the arts.

But there is another argument which at present favours the arts. Pure science—that is, science which does not have foreseeable practical applications, such as elementary particle physics or cosmology—is by and large an arcane enterprise which is appreciated mainly by its practitioners. The arts, on the other hand, are generally less restricted in their audience: many more people in the world today can gain enjoyment from listening to Beethoven's *Ninth Symphony* than they can from reading Schroedinger's paper on quantisation as an *eigen*-value problem. Granted that the intellectual delight experienced by the creator in pure science matches that of the creator in art, the direct products of the latter's efforts at present probably give more enjoyment to more people than do the products of the former. Of course, in so far as even the purest science may eventually result in practical applications, it too affects the public at large; but we are speaking here of the science whose practical application is minimal.

The well-paid pure scientists among my friends will undoubtedly object to being converted into scientific bohemians shivering in poorly heated garrets. But I don't think pure science is doomed to that poor an existence if our society decides, even now, to support it on about the same scale as it supports the arts. It is true that the arts are supported poorly by

[5] Polanyi, M., "The Republic of Science: Its Political and Economic Theory", *Minerva*, I (Autumn, 1962), 1, pp. 54–73.

government, but the total paid by the society, *i.e.*, private individuals and associations, governments, local and federal, for the arts is not negligible and the support is growing. In estimating the total support we give to the arts, we must include the value of theatre admissions, the value of books, better magazines and good records, the total that goes to our performing arts, as well as the direct subsidies in the form of grants to creative artists. The total spent by the United States on all activities that one way or another are concerned with the arts amounted in 1960 to around $2,500 million.[6] Only a fraction of this amount is spent directly by the federal government but this is not relevant. Pure science, unlike music or literature, produces no directly saleable commodity and so if it is to be supported at all by the public it must be supported by the public through its government.

Moreover, it seems likely, with the increase in leisure, and the decrease in the amount we spend on armaments, that a larger and larger fraction of our national income will go into the arts. Voices have been raised favouring a National Arts Foundation, paralleling the National Science Foundation. To make of pure science an avenue for expression of our creative intellectual energy, quite parallel to August Heckscher's[7] proposal to make of the arts such an instrument, strikes me as highly appealing. This latter viewpoint was stated eloquently by N. N. Semenov,[8] the Soviet chemist; he visualises science in the world of the future being appreciated and practised as widely as are the arts in the world today—every man a scientist, to the extent of his intellectual capacity.

I put forward the idea that the purest science be supported in the same spirit and at roughly the same level as the arts as only one among several possibilities. The arts, after all, are not the only non-scientific activity which gives deep intellectual or spiritual satisfaction. For example, religion even today gives great spiritual satisfaction to many people—in our country to many more than do the arts or sciences. And indeed, a case can be made for using the level of support of religion instead of art as a yardstick for how much pure science our society ought to support.

And yet, despite the analogies between science and art, or between science and religion, the idea of relating the degree of support of one to the degree of support of the other is somehow forced and artificial and not really satisfactory. In the long run how much our society is going to

[6] Estimate by A. Mitchell of Stanford Research Institute, as reported by *Business Week*, 19 January, 1963, p. 68.

[7] Heckscher, August, "The Arts in the 1980s", a lecture at Oswego State University, Oswego, New York (1964).

[8] Semenov, N. N., "The World of the Future", *The Bulletin of the Atomic Scientists*, XX (February, 1964), pp. 10–15; the same idea was also expressed by George Bernard Shaw in *Back to Methuselah*.

spend on basic science depends upon the extent to which non-scientists develop the intellectual power and taste to appreciate, if not to discover, science. The question then is: is it really likely that society will develop so congenial an attitude towards science—say as congenial an attitude as it now displays towards the arts or religion—that it will support the basic scientist at the level he thinks he needs?

Most scientists believe that society will be missing something very important should it *not* develop such an attitude towards pure science. Every scientist knows that much of the satisfaction he derives from his scientific career comes not only from his own original discoveries, but also from the thrill he experiences when he understands, for the first time, someone else's great discovery. My own experience during the past half dozen years illustrates the point. During these years, at least five major discoveries have been made in physics: the Mössbauer effect, the overthrow of parity, the laser, the superconducting magnet, and the SU_3 symmetry in strong interactions. After each of these discoveries I blessed my decision to study physics, since only because I knew some physics could I experience the unique intellectual satisfaction that appreciation of a discovery, almost as much as the discovery itself, affords.

One need not be a great intellect to appreciate a scientific discovery, at least enough to give one real satisfaction. I would guess that all those intelligent enough to take a university degree could learn enough to appreciate some branch of science: if not the most sophisticated parts, then at least the simpler parts. Nor is it necessary for all the public to understand all of basic science. Just as science itself has fragmented under the pressure of the information explosion, so I visualise that " lay-scientists " would also form somewhat separate communities: perhaps there might develop the equivalent of " molecular biology fan clubs ", " high-energy fan clubs ", " oceanography fan clubs ", even as we now have amateur astronomers, radio " hams " and hi-fi enthusiasts.

To educate so many people to a point where they can achieve a sense of participation in the march of science poses a major problem. The scientists themselves will have to spend much effort conveying their message, in intelligible terms, to the rest of society. They will have to deal sympathetically (much more so than I think they do now) with the scientific popularisers and with the scientific educators. If the scientists and their para-scientific associates are unable to convey this sense of scientific adventure to the community that supports them, I cannot see how the purest basic research can, in the long run, expect to receive the support it will demand in the future.

The problem faced by the future scientists has been stated by Professor Eugene Wigner as follows:

> . . . we all hope that the present competitions for the most powerful military posture will become unnecessary soon—perhaps in 10 years, perhaps in 20 years. Quite likely, not only will the present unquestioning support of science cease then; it will be replaced by distrust and even unpopularity. Nobody likes his companions and helpers of a past life from which he has turned to a better one. What will be the role of science then, where the scientist will be no longer a source of power of the government, after having been pampered so long, is not entirely pleasant to contemplate. However, it may be useful. Science that is useless in the sense that it does not help to satisfy other cravings, is still one of the noblest endeavours of man; it would be most pitiful if mankind turned away from science just when it will have the leisure to pursue science in its more noble form.[9]

Support for Basic Science as an Overhead Charge on Applied Science and Technology

I confess to a residual scepticism about our society acquiring this sophistication in the short run, which means, for the working scientist, the years until his retirement. It is probably utopian—as much as Shaw's *Back to Methuselah*—to expect every man in the street to become an amateur scientist or even a science fan.

Thus, much as I hope that our society will acquire this scientific sophistication, it seems clear that in the near, as opposed to the distant, future we shall have to present a more realistic claim on society's support of basic science done for the sheer intellectual pleasure it affords its practitioners. I therefore return to my earlier suggestion that basic science in fields clearly relevant to applied science be viewed as an overhead charge on that particular applied science—that is, against the political mission the applied science is intended to accomplish. I would extend the idea and urge that the purest basic science be viewed as an overhead charge on the society's entire technical enterprise—a burden that is assessed on the whole activity because, in a general and indirect sort of way, such basic science is expected eventually to contribute to the technological system as a whole. In some cases, the help will turn out to be direct, as when a discovery in cosmology illuminates a point in nuclear structure physics; in more cases it will be indirect as when a professor, whose research is in an abstruse field of mathematics, inspires a young engineering student with the beauties of the classical calculus of variations.

Some such view of the relation of the purest basic science to the entire technical enterprise was implicit in Executive Order 10521 issued in

[9] "Prospects in Nuclear Science", an address delivered at the Twentieth Anniversary Celebration, Oak Ridge National Laboratory, 4 November, 1963.

1954 by President Eisenhower concerning the terms of reference of the newly founded National Science Foundation:

> As now or hereafter authorised or permitted by law, the foundation shall be increasingly responsible for providing support by the federal government for general-purpose basic research through contracts and grants. The conduct and support by other federal agencies of basic research in areas which are closely related to their missions is recognised as important and desirable, especially in response to current national needs, and shall continue.

From this point of view one has further reduced the dimension of the problem of how much " science " shall we support. Applied science and engineering have already been ruled to be outside the " science as a whole " budget, inasmuch as they are a means of achieving a politically defined mission. Basic science, which is closely related to an applied science (such as biology, *vis-à-vis* medicine), is an overhead assessed against the related applied science, and therefore its level of support is again tied closely to a politically defined end. And finally, the purest basic science, viewed as an overhead against the entire enterprise, would, in analogous fashion, receive support at a level determined as a fraction of the entire remaining technical enterprise. What this fraction should be would itself be a political decision—but if all such research is supported by a National Science Foundation, as suggested by the Executive Order of President Eisenhower, this political decision would amount each year to setting the budget of the National Science Foundation. Of course this political decision would be influenced in part by the public's attitude towards science; but it would also be influenced by the attitude of legislators who are probably more inclined towards science than is the general public, since so much of the business of national legislative bodies now involves science and engineering in one way or another.

Where do the criteria of choice I proposed in my previous paper fit into such a scheme? As I see the matter now, they would be used both by mission-oriented agencies in making administrative decisions with respect to different kinds of basic science and by a body with very broad terms of reference, independently of any technological task such as those given to the National Science Foundation in the United States, in choosing between different basic fields. Within each allocation of funds made for a politically defined task there will always be more claimants than there are funds and choices will still have to be made. The beauty of the idea of basic research as a " scientific overhead " is that it reduces the size of each allocation of funds for scientific research to a more manageable proportion.

Thus I have turned a full circle: I began by asking how much " science as a whole " our society could afford. In developing my views, I have

successively reduced the magnitude of science which competes with society's other activities, first by ruling the costs of applied science to be overhead charges on the tasks it sought to further; secondly by ruling the costs of mission-related basic science to be an overhead charge on mission-related applied science; and now by suggesting that the purest science be an overhead on the entire technological system. This is not to say that I object to the view of "science as culture", a view which places science *per se* directly in competition with other activities of the society. It is merely that, in the short term, basic science viewed as an overhead charge on technology is a more practical way of justifying basic science than is basic science viewed as an analogue of art. Until and unless our society acquires the sophistication needed to appreciate basic science adequately, we can hardly expect to find in the admittedly lofty view of "science as culture" a basis for support at the level which we scientists believe to be proper and in the best interests both of society and of the scientists.

CRITERIA FOR SCIENTIFIC CHOICE[1]

Alvin M. Weinberg
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As science grows, its demands on our society's resources grow. Just as the Church in the Middle Ages placed heavy demands on Europe's gifted people, on its material wealth, and on the thoughts of the average man, so our growing science imposes similar demands on contemporary society. It seems inevitable that science's demands will eventually be limited by what society can allocate to it. We shall then have to make choices. These choices are of two kinds: we shall have to choose among different, often incommensurable, fields of science--between high-energy physics and oceanography or between molecular biology and material science; and we shall have to choose among the different institutions that receive support for science from the government--among universities, federal laboratories, and industry. The first choice I call scientific choice; the second, institutional choice. My purpose is to suggest criteria for making scientific choices--to formulate a scale of values which might help establish priorities between scientific fields whose only common characteristic is that they all derive support from the government.

Choices of this sort are made at every level both in science and in government. The individual scientist must decide what science to do, what not to do: the totality of such judgments makes up his scientific taste. The research director must choose which projects to push, which to kill. The government administrator must decide not only which efforts to support; he must also decide whether to do a piece of work in a university, a national laboratory, or an industrial laboratory. The sum of such separate decisions determines our overall policy. I shall be concerned mainly with the broadest scientific choices: how should government decide between very large fields of science, particularly between

[1]Original version presented before the Institute for Career Science-Executives, NIH, Bethesda, Maryland, October 1962. This version scheduled for publication in Minerva in 1963.

different branches of basic science? The equally important question of how government should allocate its support for basic research among industry, government laboratories, and universities will not be discussed here.

Must We Make Choices?

Most of us like to be loved; we hate to make choices, since a real choice alienates the party that loses. If one is rich--more accurately, if one is growing richer--choices can be avoided. Every administrator knows that his job is obviously unpleasant only when his budget has been cut. Thus the urgency for making scientific or institutional choices has in the main been ignored both in the United States and elsewhere because the science budget has been expanding so fast: the government spent $1.6 billion on research and development in 1950, $9 billion in 1960, $14 billion (including space) in 1962.

Though almost all agree that choices will eventually have to be made, some well-informed observers insist that the time for making the choices is far in the future. Their arguments against making explicit choices have several main threads. Perhaps most central is that we don't make explicit choices about anything else--why should we make them in science? We don't explicitly choose between support for farm prices and support for schools, or between highways and foreign aid, why should we single out science as the guinea pig for trying to make choices? The total public activity of our society has always resulted from countervailing pressures exerted by various groups representing professional specialties, or local interests, or, hopefully, concern for the public interest. The mix that emerges as our Federal budget is not arrived at self-consciously: even the highest level of authority, the President, who must weigh conflicting interests in the scale of the public interest, is limited in the degree to which he can impose an overall judgment by the sheer size of the budget if by nothing else. But because we have always arrived at an overall allocation by the free

play of countervailing pressures does not mean that such free interplay is the best or the only way to make choices. In any case, even if our choices remain largely implicit rather than explicit, they will be more sensible if people at every level, representing every pressure group, try to understand the larger issues and try to mitigate naked self-interest with concern for broader issues. The idea of countervailing and biased pressures being adjudicated at one fell swoop by an all-knowing supreme tribunal is a myth--it is much better that the choices be decentralized and that they reflect the concern for broader issues. For this reason alone philosophic debate of questions of scientific choice among scientists and administrators at all levels should lead to a more rational allocation of our resources.

A second thread in the argument of those who refuse to face the problem of scientific choice is that we waste so much on trivialities--on smoking, on advertising, on gambling--that it is silly to worry about expenditures of the same scale on what is obviously a more useful social objective--the increase of scientific knowledge. A variant of this argument is that with so much unused steel capacity or so many unemployed we cannot rightly argue that we can't afford a big cyclotron or a large manned-space venture.

In principle, there is much merit to such positions. Yet practically such arguments are not very useful. At any given instant only a certain fraction of our society's resources goes to science; for scientists to imply that the summum bonum of our society is the pursuit of science and that therefore all other activities of the society are secondary to science--that unused capacity in the steel mills should go to Big Science rather than Big Housing--is a view that will appeal strongly to the scientific community. It is hardly likely to appeal so strongly to the much larger part of our society that elects Congress, and to whom, in all probability, good houses are more important than good science. Thus as a practical matter we cannot really duck the problem of scientific choice. If those

actively engaged in science do not make choices, they will be made anyhow by the Congressional Appropriations Committee and by the Bureau of the Budget. Moreover, and perhaps more immediately, if we are not limited by money we shall be limited by the availability of truly competent men: there is already evidence that our ratio of money to men in science is too high, and that in some parts of science we have gone further more quickly than the number of really competent men can justify.

Existing Mechanisms of Choice

Our scientific and governmental communities have evolved organizational and other devices for coping with broad issues of scientific choice. The most important organizational device is the President's Science Advisory Committee with its panels and its staff, the Office of Science and Technology. This body and its panels help the Bureau of the Budget to decide what is to be supported and what is not to be supported. The panel system however suffers from a serious weakness. Panels more often than not are composed of specialized experts who inevitably share the same enthusiasms and passions. To the expert in oceanography or in high-energy physics, nothing seems quite as important as oceanography or high-energy physics. The panel, when recommending a program in a field in which all its members are interested, invariably argues for better treatment of the field--more money, more people, more training. The panel system is weak insofar as judge, jury, plaintiff and defendent are usually one and the same.

The panel is able to judge how competently a proposed piece of research is likely to be carried out: its members are all experts and are likely to know who are the good researchers in the field. But just because the panel is composed of experts, who hold parochial viewpoints, the panel is much less able to place the proposal in a broader perspective--to say whether or not the research proposal is of much interest to the rest of science. We can answer the question

"how" within a given frame of reference; it is impossible to answer "why" within the same frame of reference. It would therefore seem that the panel system could be improved if representatives, not only of the field being judged but also representatives of neighboring fields, sat on every panel judging the merits of a research proposal. A panel judging high-energy physics should have some people from low-energy physics; a panel judging low-energy physics should have some people from nuclear energy; a panel judging nuclear energy should have some people from conventional energy; and so on. I should think that advice from panels so constituted would be tempered by concern for larger issues; in particular, the support of a proposed research would be viewed from the larger perspective of the relevance of that research to the rest of science.

In addition to organizational instruments such as panels or PSAC for making choices, the scientific community has evolved an implicit method for establishing scientific priorities--that is, for deciding what is important science, what is not important. This mechanism is the scientific literature. The process of self-criticism built into the literature of science is one of the most characteristic features of science. Nonsense is weeded out and held up to ridicule in the literature, whereas what is worth-while receives much sympathetic attention in the literature. This process of self-criticism imposed by the literature, though implicit, is nonetheless real and highly significant. The existence of a healthy, viable scientific literature in itself helps assure society that the science it supports is valid and deserving of support. This is a most important, though little recognized, social function of the scientific literature.

The literature as an arbiter of scientific taste and validity is beset with two difficulties. First, because of the information explosion, the literature is not read nearly as carefully as it used to be. Nonsense is not so generally recognized as such, and the standards of self-criticism, which are so necessary

if the scientific literature is to serve as an arbiter of scientific taste, are inevitably looser than they once were.

Second, the scientific literature in a given field tends to form a closed universe; workers in a field, when they criticize each other, tend to adopt the same unstated assumptions. A referee of a scientific paper asks whether the paper conforms to the rules of the scientific community to which both referee and author belong, not whether the rules themselves are valid. So to speak, the editors and authors of a journal in a narrowly specialized field are all tainted with the same poison. As Einstein said, "Eigener Dreck stinkt nicht".[1]

Can a true art of scientific criticism be developed--i.e., can one properly criticize a field of science beyond the kind of criticism that is inherent in the literature of a field? Mortimer Taube in Computers and Common Sense (Columbia University Press, New York City, 1961) insists that such scientific criticism is a useful undertaking--that, by viewing a field from a somewhat detached point of view, it is possible to criticize a field meaningfully, even to the point of calling the whole activity fraudulent, as he does in the case of non-numerical uses of computers. I happen to believe that Taube does not make a convincing case in respect to certain non-numerical uses of computers, such as language translation. Yet I have sympathy for Dr. Taube's aims--that, with science taking so much of the public's money, we must countenance, even encourage, discussion of the relative validity and worthwhileness of the science which society supports.

Criteria for Scientific Choice

I believe that criteria for scientific choice can be identified; that in fact several such criteria already exist, and the main task is to make them more explicit. The criteria can be divided into two kinds: internal criteria and external criteria. Internal criteria are generated within the scientific field

[1]Freeman, J. Dyson, Physics Today 9, 32 (1956).

itself and answer the question, "How well is the science done?" External criteria are generated outside the scientific field and answer the question, "Why do this particular science?" Though both are important, I think the external criteria are the more important.

Two internal criteria can be easily identified: (1) Is the field ready for exploitation? (2) Are the scientists in the field really competent? Both these questions are answerable only by experts who know the field in question intimately, and know the people personally. These criteria are therefore the ones most often applied when a panel decides on a research grant: in fact, the primary consideration in offering government support to a scientist is usually "How good is he?"

But I believe it is not tenable to base our judgments entirely on internal criteria. As I have said, we scientists like to believe that pursuit of science in itself is society's highest good, but this view cannot be taken for granted. For example, we now suffer a serious shortage of medical practitioners, probably to some extent because many bright boys who would formerly have gone into medical practice now go into biological research: government fellowships are generally available for graduate study leading to the Ph.D. but not for study leading to the M.D. It is by no means self-evident that society profits most from more biological research and less medical practice. I would much rather have my appendix removed by a surgeon who made A in his medical school courses than wait for him to discover a cure for appendicitis. Society does not a priori owe the scientist, even the good scientist, support any more than it owes the artist or the writer or the musician support. Science must seek its support from society on grounds other than that the science is done competently and that it is ready for exploitation; scientists cannot expect society to support science because they (the scientists) find it an enchanting diversion. Thus in seeking justification for support of science, we are led inevitably to consider external criteria for the

validity of science--criteria that stem from outside science, or outside a given field of science.

External Criteria: Technological Merit

Three external criteria can be recognized: technological merit, scientific merit and social merit. The first is fairly obvious: once we have decided, one way or another, that a certain technological end is worthwhile, we must support the science necessary to achieve that end. Thus if we have set out to learn how to make breeder reactors, we must first measure painstakingly the neutron yields of the fissile isotopes as a function of energy of the bombarding neutron. As in all such questions of choice, it is not always so easy to decide the technological relevance of a piece of basic research. The technological usefulness of the laser came after, not before, the principle of optical amplification was discovered. But it is my belief that such technological bolts out of the scientific blue are the exception, not the rule. Most programmatic basic research can be related fairly directly to a technological end at least crudely if not in detail. The broader question of whether the technological aim itself is worthwhile must be answered again partly within technology (is the technology ripe for exploitation, are the people any good?), partly outside technology (are the social goals worthwhile?). Many times these questions are difficult to answer, and sometimes are answered incorrectly: the United States launched an effort to control thermonuclear energy in 1952 on a rather large scale because it was thought at the time that controlled fusion was much closer at hand than it turned out to be. Nevertheless, despite the fact that we make mistakes, technological aims are customarily scrutinized much more closely than are scientific aims; we at least have more practice discussing technological merit than we do scientific merit.

External Criteria: Scientific Merit[2]

The criteria of scientific merit and social merit are much more difficult: scientific merit because we have given little thought to defining "scientific merit" in the broadest sense, social merit because it is difficult to define the social aims of our society. As I have already suggested, the answer to the questions "Does this broad field of research have scientific merit?" cannot be answered within the field. The idea that the scientific merit of a field can be judged better from the vantage point of the scientific fields in which it is embedded than from the point of view of the field itself is implicit in the following quotation from the mathematician, J. von Neumann: "As the (discipline) travels far from its empirical source, as it loses its contact with the sister sciences (in which every science at one time is embedded), it is beset with very grave dangers. It becomes more and more pure aestheticizing, more and more purely l'art pour l'art. This need not be bad if the field is surrounded by correlated subjects which still have closer empirical connections or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much 'abstract' inbreeding, a mathematical subject is in danger of degeneration. At the inception the style is usually classical; when it shows signs of becoming baroque, then the danger signal is up."[3]

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[2]This section and the section on "Applications" were first presented, in somewhat different form, as the Phi Kappa Phi Guest Lecture at The University of Tennessee, January 25, 1962, and appeared in the Phi Kappa Phi Journal, Summer 1962.

[3]Robert B. Heywood, editor, The Works of the Mind, p. 195, University of Chicago Press, Chicago (1947).

I believe there are any number of examples to show that von Neumann's observation about mathematics can be extended to the empirical sciences: that empirical basic sciences which move too far from the neighboring sciences in which they are embedded tend to become "baroque", and that relevance to neighboring fields of science is therefore a valid measure of the scientific merit of a field of basic science. Insofar as our aim is to increase our grasp and understanding of the universe, we must recognize that some areas of basic science do more to round out the whole picture than do others. A field in which lack of knowledge is a bottleneck to the understanding of other fields deserves more support than a field which is isolated from other fields. This is only another way of saying that, ideally, science is a unified structure and that scientists, in adding to the structure, ought always to strengthen its unity. Thus the original motivation for much of high-energy physics is to be sought in its elucidation of low-energy physics, or the strongest and most exciting motivation for measuring the neutron capture cross sections of the elements lies in the elucidation of the cosmic origin of the elements. Moreover, the discoveries which are acknowledged to be the most important scientifically have the quality of bearing strongly on the scientific disciplines around them. For example, the discovery of X ray was important partly because it extended the electromagnetic spectrum but, much more, because it enabled us to see so much that we had been unable to see. The word "fundamental" in basic science, which is often used as a synonym for "important", can be partly paraphrased to "relevance to neighboring areas of science". I would therefore sharpen the criterion of scientific merit by proposing that, other things being equal, that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighboring scientific disciplines. This is the justification for my previous suggestion about making it socially acceptable for people in related fields to offer opinions on the scientific merit of work in a given field.

In a sense, what I am trying to do is to extend to basic research a practice that is customary in applied science: a project director trying to get a reactor built on time is expected to judge the usefulness of component development and fundamental research which bears on his problems. He is not always right; but his opinions are usually useful both to the researcher and to the management disbursing the money.

External Criteria: Social Merit

I turn now to the most controversial criterion of all--social merit or relevance to human welfare and the goals of man. Two difficulties face us when we try to clarify the criterion of social merit: first, who is to define the goals of man, or even the goals of our own society; and second, just as we shall have difficulty deciding whether a proposed research helps other branches of science or technology, so we shall have even greater trouble deciding whether a given scientific or technical enterprise indeed furthers our pursuit of social goals, even when those goals have been identified. With some goals we have little trouble: adequate defense, or more food, or less sickness, for example, are rather uncontroversial. Moreover, since such goals themselves are relatively easy to describe, we can often guess whether a scientific activity is likely to be relevant, if not actually helpful, in achieving the goal. On the other hand, some social goals are much harder to define: perhaps the most difficult is national prestige. How do we measure national prestige? What is meant when we say that a man on the moon enhances our national prestige? Does it enhance our prestige more than, say, discovering a polio vaccine or winning more Nobel Prizes than any other country? Whether or not a given achievement is prestigious probably depends as much on the press-agentry that accompanies the achievement as it does on its intrinsic worthwhileness.

Among the most attractive social goals that science can help achieve is international understanding and cooperation. That science often transcends political boundaries needs little elaboration. The new element injected by the advent of Big Science is that now it is prudent as well as efficient to do science cooperatively. The very big accelerators are so expensive that international laboratories such as CERN at Geneva spring up to enable different countries to share costs that are too heavy for them to bear separately. Even if we were not committed to improving international relations, we would be impelled to cooperate merely as a means of saving money.

Bigness is an advantage rather than a disadvantage if science is to be used as an instrument of international cooperation: a half-billion dollar cooperative scientific venture--such as the proposed 1000 Bev intercontinental accelerator--is likely to have more impact than a half-million dollar Van de Graaff machine. The most expensive of all scientific or quasi-scientific enterprises--the exploration of space--is from this viewpoint the best-suited instrument for international cooperation. The exchange between President Kennedy and Chairman Khrushchev concerning possible increased cooperation in space exploration seems to have been well-received and, one hopes, will bear ultimate fruit.

Applications

Having set forth these criteria and recognizing that judgments are fraught with difficulty, I propose to assess five different scientific and technical fields according to the aforementioned criteria. The five fields I choose are molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioral sciences. Two of these fields, molecular biology and high-energy physics, are, by any definition, basic sciences; nuclear energy is applied science, the behavioral sciences are a mixture of both applied and basic science. Manned exploration of space, though it requires the tools of science and is regarded

in the popular mind as being part of science, has not yet been proved to be more than quasi-scientific, at best. The fields which I choose are incommensurable: how can one measure the merit of behavioral sciences and nuclear energy on the same scale of values? Yet the choices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they can be applied to seemingly incommensurable situations. The validity of my proposed criteria depends on how well they can serve in comparing fields that are hard to compare.

Of the scientific fields now receiving public support, perhaps the most successful is molecular biology. Hardly a month goes by without a stunning success in molecular biology being reported in the Proceedings of the National Academy of Sciences. The most recent has been the cracking by Nierenberg and Ochoa of the code according to which triples of bases determine specific amino acids in the living proteins. Here is a field which rates the highest grades as to its ripeness for exploitation and competence of its people. It is profoundly important for large stretches of other biological sciences--genetics, cytology, microbiology--and therefore, according to my criterion, must be given A+ for its scientific merit. It also must be given a very high grade in social merit, and probably in technological (that is, medical) merit--more than, say, taxonomy or topology. Molecular biology is the most fundamental of all the biological sciences. With understanding of the manner of transmission of genetic information ought to come the insights necessary for the solution of such problems as cancer, birth defects, and viral diseases. Altogether, molecular biology ought, in my opinion, to receive as much public support as can possibly be pumped into it; since money is not limiting its growth, many more graduate students and fellows in molecular biology ought to be subsidized so that the attack on this frontier can be expanded as fast as possible.

The second field is high-energy physics. This field of endeavor originally sought as its major task to understand the nuclear force. In this it has been only modestly successful; instead it has opened an undreamed of subnuclear world of strange particles and hyperons, a world in which mirror images are often reversed. The field has no end of interesting things to do, it knows how to do them, and its people are the best. Yet I would be bold enough to argue that, at least by the criteria which I have set forth--relevance to the sciences in which it is imbedded, relevance to human affairs, and relevance to technology--high-energy physics rates poorly. The nuclear forces are not being worked on very directly--the world of subnuclear particles seems to be remote from the rest of the physical sciences. Aside from the brilliant resolution of the τ-particle paradox which led to the overthrow of the conservation of parity, and the studies of mesic atoms (the latter of which is not done at ultra-high energy), I know of few discoveries in ultra-high-energy physics which bear strongly on the rest of science. (This view would have to be altered if machines such as the Argonne Zero Gradient Synchrotron were exploited as very strong, pulsed sources of neutrons for study of neutron cross sections.) As for its bearing on human welfare and on technology, I believe it is essentially nil. These two low grades would not bother if high-energy physics were cheap. But it is terribly expensive--not so much in money as in highly qualified people, especially those brilliancies who could contribute so ably to other fields which contribute much more to the rest of science and to humanity than does high-energy physics. On the other hand, if high-energy physics could be made a vehicle for international cooperation--if the much discussed intercontinental 1000 Bev accelerator could indeed be built as a joint enterprise between East and West--the expense of high-energy physics would become a virtue, and the enterprise would receive a higher grade in social merit than I would now be willing to assign it.

Third is nuclear energy. This being largely an applied effort, it is very relevant to human welfare. We now realize that in the residual uranium and thorium of the earth's crust mankind has an unlimited store of energy--enough to last for millions of years; and that with an effort only one-tenth of our manned-space effort we could, within ten or fifteen years, develop the reactors which would tap this resource. Only rarely do we see ways of permanently satisfying one of man's major needs--in this case energy. In high-conversion-ratio nuclear reactors we have such means, and we are close to achieving them. Moreover, we begin to see ways of applying very large reactors of this type to fulfill one of mankind's dreams--economical desalination of the ocean. Thus the time is very ripe for exploitation. Nuclear energy rates so highly in the categories of technical and social merit and timeliness that I believe it deserves strong support, even if it got failing grades in the other two categories--people and relation to the rest of science. Suffice to say that in my opinion the nuclear energy people are good and that nuclear energy in its basic aspects has vast ramifications in other scientific fields.

Next on the list are the behavioral sciences--psychology, sociology, anthropology, economics. The people are bright; the sciences interrelate well with each other; the sciences are deeply germane to every aspect of human existence. In these respects the sciences deserve strong public support. On the other hand, it is not clear to me that the behavioral scientists, on the whole, see clearly how to attack the important problems of their sciences. Fortunately the total sum involved in behavioral science research now is relatively tiny--as it well must be when what are lacking are deeply fruitful, and generally accepted, points of departure.

Finally I come to manned-space exploration. The personnel in the program are competent and dedicated. With respect to ripeness for exploitation, the

situation seems to me somewhat unclear. Our hardware is in good shape, and we can expect it to get better--bigger and more reliable boosters, better communication systems, etc. What is not clear is the human's tolerance to the space environment. I do not believe that either the hazards of radiation or the weightlessness are yet sufficiently explored to positively guarantee success in our future manned-space ventures.

The main objection to spending all the manpower, not to say money, on manned-space exploration is its remoteness from human affairs, not to say the rest of science. In this respect space (the exploration of very large distances) and high-energy physics (the exploration of very small distances) are similar, though high-energy physics has the advantage of greater scientific validity. There are some who argue that the great adventure of man into space is not to be judged as science, but rather as a quasi-scientific enterprise, justified on the same grounds as we justify other non-scientific national efforts. The weakness of this rationalization is that space requires many, many scientists and engineers, and these are badly needed for such matters as clarifying our civilian defense posture or, for that matter, working out the technical details of arms control and foreign aid; if space is ruled to be non-scientific, then it must be balanced against other non-scientific expenditures like highways, schools, or civilian defense. If we do space because of prestige, then we should ask whether we get more prestige from a man on the moon than from successful control of the water-logging problem in Pakistan's Indus Valley Basin. If we do space because of its military implications, we ought to say so--and perhaps the military justification, at least for developing big boosters, is plausible as the Soviet experience with rockets makes clear.

External Criteria and the Aims of Science

The main burden of my argument is that the most valid criteria for assessing scientific fields come from without rather than from within the scientific discipline that is being rated. This does not mean that only those scientific fields deserve priority that have high technical merit or high social merit. Scientific merit is as important as the other two criteria but, as I have argued, scientific merit must be judged from the vantage point of the scientific fields in which each field is embedded rather than from that of the field itself. If we support science in order to maximize our knowledge of the world around us, then we must give the highest priority to those scientific endeavors that have the most bearing on the rest of science.

The rather extreme view which I have taken presents difficulties in practice. The main trouble is that the bearing that one science has on another science so often is not appreciated until long after the original discoveries have been made. Who was wise enough, at the time Purcell and Bloch first discovered nuclear magnetic resonance, to guess that the method would become an important analytical tool in biochemistry? Or how could one have guessed that Hahn and Strassmann's radiochemical studies would have led to nuclear energy? And indeed, my high-energy physics friends imply that what we learn about the world of strange particles will in an as yet indiscernible way teach us much about the rest of physics, not merely much about strange particles; they beg only for time to prove their point.

To this argument I say first that choices are always hard; it would be far simpler if the problem of scientific choice could be ignored, and possibly in some future millennium it can be. But there is also a more constructive response. The necessity for scientific choice arises in Big Science, not in little science. Just as our society supports artists and musicians on a small scale, so I have no objection to--in fact, I strongly favor--our society's supporting science that rates

zero on all the external criteria, provided it rates well on the internal criteria (ripeness and competence) and provided it is little science. It is only when science really does make serious demands on the resources of our society--when it becomes Big Science--that the question of choice arises at all.

At the present time, with our society faced with so much unfinished and very pressing business, science can hardly be considered the major business of our society. For scientists as a class to imply that science can at this stage in human development be made the main business of humanity is irresponsible--and, from the scientist's point of view, highly dangerous. It is quite conceivable that our society will tire of devoting so much of its wealth to science, especially if the implied promises held out when big projects are launched do not materialize in anything very useful. I shudder to think what would happen to science in general if our manned-space venture turned out to be a major flop--if it turned out, for example, that man could not withstand the re-entry deceleration forces after a long sojourn in space. It is as much for this primitive reason--survival--as for any loftier motive that scientists must acquire the habit of scrutinizing what they do from a broader point of view than has been their custom. To do less could cause a popular reaction which would destroy mankind's most remarkable intellectual edifice--Modern Science--or at least the caste--the scientists--that created it.

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U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

Scientific Advice for Congress

A veteran legislator suggests that current proposals are overlooking some realities of legislative life.

Clinton P. Anderson

One of the results of the growing federal involvement in science and technology has been a growing uneasiness in Congress about its own ability to oversee programs in these areas effectively. The number of inquiries into the general state of science-government relationships undertaken recently is a measure of this unrest, as is the variety of proposals put forth to improve Congress's capacity to judge scientific programs. There is no doubt that Congress does have to make some adjustments to changing patterns of federal expenditure, and all the proposals deserve to be taken seriously. But before a wholly new system for dealing with science is created, it would be well to examine both the source of Congressional interest in science and the kind of advisory structure best suited to its needs.

There are at least three reasons for the interest of Congress in improving its grasp of science and technology. The first is cost consciousness—this year's federal R&D budget is about $15 billion. Congress is concerned, however, not only about the amount of money spent on research and development (which has multiplied 100-fold since 1940) but about the relationship of cost to performance. How can Congress make intelligent decisions when budget costs are based on estimates which fail to hold true? The Air Force, for example, estimated in 1960 that Project Skybolt would cost $893 million; in 1961 the estimated cost had reached $1.9 billion, and by the summer of 1962—when Skybolt was scrapped—not only had the cost estimate climbed to $2.3 billion, but Skybolt was a year and a half behind schedule. Another example is the project for the nuclear-powered airplane (ANP). In November 1951, one contractor estimated that it would take $188 million to deliver the nuclear power plant for mounting in an aircraft by May 1956. By 1961, when the project was cancelled, the costs of that one company had reached over $527 million and the power plant had never been delivered. The total cost of ANP, when it was ended, exceeded $1 billion.

It is true that the money supposedly "wasted" on the nuclear-powered plane may yet pay valuable dividends when some of its positive findings in metallurgy and instrumentation are applied to some future project, such as the supersonic airliner. Knowledge, however useless at the moment of its discovery, will someday find its place in the scheme of things and make its contribution. Nonetheless, a better way must be found to estimate the long-range costs of R&D programs; more accurate target dates for their completion must be determined. And Congress needs to be more accurately informed on both, not only for their implications for the budget and the sensible allocation of funds for R&D, but for their frequent implications for national defense as well.

Legislative Control

A second reason for Congressional attention to what Vannevar Bush has called the "endless frontier" is the belief among some members that Congress has lost the ability to oversee effectively the vast diffusion of R&D activities for

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which it appropriates funds. As proof, take the statement of Senator E. L. Bartlett (D–Alaska) when he recently proposed the creation of a Congressional Office of Science and Technology: ". . . At the present time," he said, "the Congress does not appreciate the importance of scientific decisions and as a result they are made, not in the Halls of Congress, but elsewhere, not by the elected representatives but by unknown administrative officials. . . . How is a popular elected government to control its own activities? How are elected officials to direct development of something they do not understand with implications they do not comprehend?" These questions go to the heart of our representative system.

And, third, there is concern that the procedures of Congress may not measure up to the demands of "big science." New techniques for obtaining information may be required so that Congress will approach parity of knowledge with the executive agencies —in other words, that Congress will have its own sources of accurate information apart from the agencies and that this source of information will better enable Congress to judge the merits of any particular research and development project.

All these concerns are serious, and I certainly agree that Congress needs advice on scientific and technical matters. But before we go about setting up a system, it is important to clarify the definition of what "scientific advice" is, and to figure out what kind of scientific advice Congress needs.

"R&D" Distinction

Of the $15 billion of federal expenditure which too loosely gets labeled as spending for science, only $1.5 billion is for basic research. Another $1.2 billion is for research and development facilities; and $12.3 billion is for developmental hardware—not science, but engineering and technology. Most of this spending is accounted for by the revolutionary changes in defense systems which have taken place within the last decade. On these hardware items, engineers can give better estimates of cost and time than the producers of the scientific concept. The first point, therefore, is that advice on engineering must be included in the definition of scientific advice.

Another important consideration is that Congress needs to look on science not as an independent function such as agriculture or defense, but simply as a factor to be weighed in the solution of a variety of problems.

When Congressmen look at the test ban treaty, or water pollution by synthetic detergents, or the NASA authorization bill, they see issues of public policy on which decisions are made not alone, or even primarily, on the basis of technical factors, but also on many other considerations as well—administrative, economic, political, and social.

Ninety percent of the approximately $8 billion the Defense Department spends for research and development goes to produce hardware for better transport, communications, weapons, and other equipment to give the military the wherewithal to fulfill its approved missions. Knowledge of science and technology is not required for Congress to determine whether this money is being spent in consonance with assigned defense responsibilities. The executive examines in great detail the way in which the Defense Department should operate; the detailed justification of the Defense budget reveals to the congressional committees what is hoped to be achieved with the funds. This can be measured against congressional understanding of military missions. Congress, for example, is fully capable of determining the roles of the Air Force and NASA in the total space program. It can weigh the broad missions of the Air Force and how best to accomplish them. These are neither scientific nor technical questions.

The kind of advice and information needed by Congress varies. At times, particularly where the major factor is technical in nature, we need the advice of the most prominent scientists we can obtain. A discussion by knowledgeable scientists of the earth orbit versus the lunar orbit as the best way to get men to the moon, for example, would have helped us better understand the choices before us, the limitations of the alternatives, and the probabilities of success or failure. A panel, on occasion, could assist in reviewing a particular segment of an agency's program, such as the adequacy of NASA's provisions for space sciences or the basic research part of the defense R&D budget. But I doubt if Congress could usefully employ such eminent scientists full time.

Needed: The "Generalist"

What Congress needs most, it seems to me, is the advice of the well-rounded "generalist" who, having a scientific or engineering background, is familiar with the workings of the Federal Government, and with a number of executive agency R&D programs, particularly with their management. Experience in coordinating the work and projects of others in terms of the over-all mission or goal would be valuable. He should be familiar with the scientific and technical community, so that he will know where to seek help when it is needed. He must have an appreciation of the values and ways of the legislative process, a feeling for public policy, and a capacity for sorting out public issues, competing values, and alternative solutions. Additionally, we need a person whose engineering background enables him to give us sound judgment on the costs of a project.

Proposals for a single source of advice to Congress do not take sufficient account of the committee structure. Each committee is restricted in interest and scope of responsibility, yet many areas of congressional interest cut across several fields. For example, we cannot really review water research and development, or oceanographic research, or total basic research, or scientific manpower resources, without cutting across committee responsibilities and looking at many executive departments. Likewise, scientific and technical advice is required from many disciplines. For Congress, or the Senate itself, to have a staff in a position to answer all of the inquiries of the various members and committees would require a duplication of the staffs within the executive agencies. It would require people with detailed knowledge of the missions and programs of all of the executive departments. This is impractical, it is too costly, and it has never been the intent of Congress. Furthermore, I do not think Congress needs it.

Filling the Need

I do not see how three or four scientists and engineers can provide even the Senate with the quality and quantity of advice needed by its committees. The demands on both their time and their talent would be too

great. Further, the general terms, science and technology, need to be broken down into scientific disciplines before we can analyze what kind of scientists and engineers we are talking about and whether or not they could meet our needs. How could a biologist, a chemist, and a physicist, either separately or in combination, assist the Senate Committee on Aeronautical and Space Sciences in determining whether to authorize funds for a deep space probe or a specific type of communication satellite system? If the physicist has a space background which enables him to be useful to one committee, then his time would presumably be taken up with that committee work and he would not be available to other committees. Similarly, if the biologist were busy assisting the Committee on Agriculture, he would not be available to help other committees during the time when hearings were being held simultaneously by several committees. How can one biologist assist with problems of pesticides, the pollution of air, land, and water, manned spaceflight, or radioactive isotopes for cancer research? For that matter how can any single man be the repository of all relevant knowledge about his own discipline? Men who are experts in naval reactors are not necessarily qualified to advise even on reactors for space propulsion. If the function of these experts is only to put us in touch with other experts, I should like to point out that this is what our permanent committee staff is already doing.

In the last analysis it is the collective wisdom of Congress itself which counts most in making important decisions. No decisions can be made in isolation, on a completely scientific basis, by disinterested officials. Congress will consider the scientific aspects of a proposal and pay attention to the facts assembled by the engineer. But in addition, Congressmen must ask some further questions: What will the impact be on our economy? What effect will the proposal have on our foreign relations? Will it contribute to the health and welfare of the nation?

It is said that Congress, because it has maintained certain rituals for years, is a 19th century body faced with 20th century problems. I disagree. Precedents and practices of Congress may have been maintained that are perhaps archaic in this age of science and technology. But the minds of Congressmen are products of the 20th century. There is no relationship between the rituals maintained by an institution and its mental capability. Congress could legislate as well in the 20th century if its members still wore powdered wigs and capes instead of Ivy League clothes.

Congressional Initiative

There are numerous examples from the area of atomic energy when Congress spurred momentous decisions, in the face of inconclusive advice from experts, which have withstood the challenge of history and have proved right:

1) The decision to proceed with the development of the hydrogen bomb against the advice of the General Advisory Committee of the Atomic Energy Commission;

2) The decision to plan a broad weapons program which required the development of large quantities of fissionable materials, even though predictions were that this country could never provide the uranium-235 and plutonium needed;

3) The development of the *Nautilus* and the nuclear submarine fleet, against determined opposition;

4) The development of a variety of power reactors.

Early in the 1950's there was some discussion about the potential benefits of multiple purpose reactors. Coming from the arid southwest, I had some acquaintance with the problem of developing new sources of water for a rapidly growing population, and I found the possibilities quite fascinating. As a member of the Senate Interior Committee, I had had a hand in legislation accelerating the work of the Office of Saline Water in demonstrating techniques for converting brackish and sea water into potable water; I knew that the drawback of known conversion processes was that the expense of the large energy requirements for desalinization made the end product economically unattractive.

In 1955, in response to a request, I received a letter from a technical employee of the Los Alamos Scientific Laboratory outlining the potential benefits of a multipurpose reactor. I was not interested in, and certainly not qualified to judge, the "how" of the reactor. I was interested in the "why" of the concept, and whether we should invest in its development.

Reactor Proposal

The letter from Los Alamos, written in simple English, described a type of reactor with three different characteristics: it would produce electrical energy, it would breed more fuel than it consumed, and its by-product heat could be used to distill saline water. Clearly, there was a good deal of economic appeal in this. But reactor technology then was not up to the task. Some congressional prodding was required to get the AEC to move forward with studies of multipurpose reactors.

As a result of that prodding we will in time develop nuclear electrical energy at a cost of a 1½ or 2 mills per kilowatt hour and water at a cost of about 15 cents per 1000 gallons instead of the present cost of $1.25. This will be a practical result arising from the action of practical men urged on by scientists who are called in by a member of Congress for advice, but who do not become members of a congressional staff.

Perhaps this illustrates how a legislator can help shape—I hope intelligently—decisions on science and technology. The process of cross-pollination, exposure to a range of problems through various committee assignments, can supplement the advice of experts in helping Congressmen reach decisions. So can the process of osmosis, through which, over a period of time, members of Congress, through their committee assignments and awareness of the world around them, absorb some familiarity with the language and problems of scientists and technicians. Since science is only one factor in shaping the good society, I would paraphrase Clemenceau: science is too important to be left solely to the scientists.

I do not want to leave the impression that Congress has been infallible in its decisions on science and technology. Congress has made mistakes. In many cases, it has pushed programs too hard. But our scientific advisers have also made misjudgements. And we cannot count on one group to do the whole, difficult job.

Instead, we should try in a variety of ways to overcome the problems involved in the relationship of Congress with the "endless frontier."

1) We should strengthen the staffing of all committees which deal with science.

2) These committees should make intelligent use of *ad hoc* groups to give counsel on technical problems.

3) There should be an easier flow of information among the congressional committees themselves so that Congress avoids needless duplication in repetitious hearings and over-burdening of witnesses.

4) Representatives of the executive agencies should improve their method of presentation to congressional committees. In discussing purely scientific problems, there is no coloration of "executive" or "legislative" science. It is science for the nation as a whole. There are a limited number of people available with the broad knowledge necessary to give Congress advice on purely scientific questions. Although the Office of Science and Technology is an arm of the President, it would be most helpful if its staff could testify fully and adequately before congressional committees. The separation of legislative and executive powers in this regard can be carried to an extent that does damage to programs in which both branches have a mutual interest.

5) The channels for gathering information through the Legislative Reference Service of the Library of Congress should be expanded, and greater use should be made of such existing organizations as the National Academy of Sciences–National Research Council and the National Science Foundation.

6) Congress should receive an annual report on the state of science and technology. Each year we receive from the President a message on the State of the Union, a Budget Message, and various other reports. The President transmits to us through the National Aeronautics and Space Council a report on the year-long activities in space and aeronautics. Perhaps the National Academy of Sciences, through its various committees, could prepare a report by itself or in association with others such as the Office of Science and Technology. The report would briefly discuss the major programs in science and technology and would set forth what problems might be on the horizon which would require congressional attention. Separately, but more effectively, in conjunction with the National Academy, the National Society of Professional Engineers might report on the state of engineering since engineering is such a large part of government R&D programs.

There are no magic ways or easy devices to solve the problem of providing Congress with adequate advice on science and technology. Any approach that some would view as ideal would still be a long way from perfection and could also produce undesirable effects upon both science and government. As H. L. Mencken said: "An idealist is one who, on noticing that a rose smells better than a cabbage, concludes that it will also make better soup."

But those who are the doers of science, and we, in political life, have a mutual responsibility to improve the relationship of Congress and the "endless frontier." As concerned individuals and collectively as members of society, we have a stake in this task.

SCIENCE AND CONGRESS

by Kenneth Kofmehl
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Prepared for delivery at the 1964 Annual Meeting of The Midwest Conference of Political Scientists, Madison, Wis. University of Wisconsin April 30-May 2, 1964

> Now is the winter of our discontent
> Made glorious summer by this sun of York. King Richard III
> Act I, Sc. 1, Line 1

To paraphrase the Bard, 1963 was the winter of congressional discontent with science. In January there were intimations of impending cuts in expenditures for research and development. Congressional intent to scrutinize specific projects more closely was indicated by the extension of authorization requirements to the research and development activities of the Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration. As the session wore on, criticism of these agencies and others administering scientific programs, like the National Institutes of Health and the National Science Foundation, mounted in crescendo. A campaign to improve the ways in which Congress gets scientific advice was launched. And the demand for a congressional probe into the organization, conduct, and total outlay for federal scientific activities became more and more insistent.

Rather surprisingly, this widespread manifestation of displeasure burst out after more than two decades of generous treatment by Congress, which had made the federal government the preeminent patron of science and technology in this country. Verifying Don Price's thesis about the impact of consensuses among scientists on the content and mode of administration of public policy,[1] Congress had established the Atomic Energy Commission, National Science Foundation, National Aeronautics and Space Administration, and Office of Science and Technology for the President largely in accordance with their wishes. These agencies embodied the dominant views of the scientific community on organization, procedures, and policies--such as the desirability of civilian control of atomic energy, peaceful uses of outer space, and a commitment to federal support for basic research and advanced scientific education. Moreover, companion congressional bodies like the Joint Committee on Atomic Energy and the science and space committees pressed for an expansion of some of their programs

more vigorously than the Administration. From 1957 on, in response to the indulgent initiatives of the chairmen of the cognizant appropriations subcommittees, Representative John E. Fogarty (D.-R.I.) and Senator Lister Hill (D.-Ala.), Congress had regularly appropriated more than the budget had requested for the National Institutes of Health--sometimes even more than the NIH was able to spend prudently, with a consequent return to the Treasury of unused funds.

What was responsible for this sharp reaction of Congress toward science? As might be expected, a multiplicity of factors interacted. Paramount among them was the skyrocketing growth in federal expenditures for science. The $2.9 billion increase in expenditures for research and development from fiscal year 1963 to fiscal year 1964 was the largest one-year rise in the history of federal spending for science. And this was but the latest in a succession of sharp increases. The amount spent on research and development had grown from $3.15 billion and 4.7 percent of the total budget in fiscal 1954 to $14.88 billion and 15.1 percent of all federal expenditures in fiscal 1964. Going back to the first year for which fairly reliable data are available, fiscal 1940, affords an even more dramatic contrast. In that year, federal funds devoted to R & D were only $74 million and constituted but .3 percent of the total budget.

Even more significant than the magnitude was the exponential rate of this growth. Between fiscal 1940 and fiscal 1964, the increase in federal R & D expenditures averaged 19.8 percent per year very consistently. Obviously, this trend could not continue much longer. For were this rate of growth to persist unabated, the entire federal budget would be devoted to R & D by around the year 1980. And the projected federal R & D expenditures for fiscal 1965 indicated that the rate of growth might already be leveling off. The $15.29 billion estimate for fiscal 1965 was $2.5 billion under what a continuation of the 19.8 percent per year trend would have provided. In any event, knowledgeable persons in the executive branch, Congress, and scientific community became aware that a downward adjustment in the growth rate of federal R & D spending was imminent. And this generated much concern about the need for developing a better means of assigning priorities to the various scientific enterprises supported by federal funds.[2]

A closely related consideration was the economy drive by fiscal conservatives of both parties. The House Republicans formed a task force, which included eleven Republicans from the House Appropriations Committee and was aided by former Budget Director, Maurice Stans, to study the budget and recommend reductions. On March 4, 1963, Representative Frank T. Bow (R.-Ohio), chairman of this task force, announced that $4 to $6 billion should be cut from the proposed expenditures. Representative Clarence Cannon (D.-Mo.), chairman of the House Appropriations Committee, avowed a goal of slicing some $5.4 billion from the budget, which he said was "loaded with waste." And the economy bloc freely admitted that a motive behind the unprecedented delay in enacting appropriations measures was to hold down federal spending.[3] The R & D component of the budget attracted their special attention not only because of recent rapid growth but also because of being far more vulnerable than fixed or well-entrenched items, such as interest on the public debt, veterans' benefits, and agricultural subsidies.

Geographic distribution of funds projected federal R & D programs into congressional consciousness from another direction. About two thirds of federal R & D expenditures are made through contracts with private industry; over ten percent through grants and contracts with universities and other nonprofit institutions; and the rest by government personnel in federal facilities. The competitive struggle among regions for larger shares of these funds has enriched our political vocabulary with the term "pork barrel science." Some of the most bitter battles have been waged over the location of university research centers and federal facilities. For these have a sort of "multiplier effect." Besides contributing directly to the economy of their locale, they attract additional science-based industry, which in turn can obtain more federal R & D contracts in a cumulative upward spiral of prosperity. A current example which should be familiar to this Midwestern audience, if not too painful to contemplate, is the MURA high energy accelerator, which would have cost about $170 million to build and would have had operating costs of about $30 million a year. Perhaps more familiar on the national scene has been the controversy over the site of the $50 million NASA Electronics Research Center--a competition which Purdue entered belatedly and unsuccessfully. An equally famous illustration is afforded by the three-way tug of war between North Carolina, West Virginia, and Maryland for the $33 million PHS Environmental Health Center, which has delayed construction of that installation for more than three years.

The impact of federal R & D spending on universities and colleges spawned an analogous cluster of problems. The flow of federal funds threatened not only to enhance the sciences at the expense of the humanities and social sciences but also to favor certain fields like high-energy physics and molecular biology among the natural sciences. Diversity in practices among federal agencies in awarding research grants and contracts meant that the recipient institutions often might not receive full reimbursement for the indirect costs involved. There was fear that dependence of some universities on the federal government for a large share of their operating costs would impair their academic integrity and independence. Preoccupation of top faculty members with obtaining and administering research projects caused undergraduate training in the large universities to suffer. Without the increasingly expensive equipment required and the higher faculty salaries made possible by federal support, small colleges were in danger of being priced out of the market of producing science graduates. The practice of awarding federal grants and contracts on the basis of existing capabilities resulted in a concentration of federal monies in ten or so leading universities, further aggravating disparities in talent and resources. Some critics contended that the primary factor responsible for this pattern of awards was the system of advisory panels used by the federal agencies in making research grants and contracts. Comprised of faculty members from the big, prestige universities, these panels tended to favor the applications of their colleagues. Summing up this charge very neatly, John Walsh writes:

> Perhaps the chief advantage of the big, rich universities in federal research is that they have Washington representatives of the most effective kind in the faculty members who sit on science agency policy committees and project selection panels. They know each other and they know the science agency bureaucrats, and there is no question that a kind of freemasonry prevails among scientists in the major research centers. Inside observers say that, while panelists strain for objectivity in judging research projects, applications from prestige institutions enjoy a subliminal advantage.[4]

This issue of favoritism in making research awards was but a part of the much larger question of how the federal government could best bring to bear the opinion and knowledge of the scientific community in the formulation of public policy. Although rather badly overdrawn and incorporating some dubious premises,

the picture Meg Greenfield painted in "Science Goes to Washington" conveys impressionistically the bases for concern on this score in the executive branch: the confusing maze of advisory bodies; the overlapping memberships among them, the universities, private industry, and professional associations; the difficulty in determining which hat a scientist who occupies multiple positions is wearing; the intramural feuding among scientists; the potential abuses which the secrecy surrounding these bodies may cloak; and the consequent impossibility of assigning responsibility for scientific counsel in the government.[5] Disgruntled scientists and industrialists fanned these anxieties by publicly complaining that dissenting opinions did not have an adequate opportunity to be heard in the process by which the executive branch obtained scientific advice on which to base major decisions.

Nevertheless, whatever the shortcomings in the system, through the panels of the Presidential Science Advisory Committee, the Federal Council on Science and Technology, and the National Academy of Sciences-National Research Council, the Administration had recourse to advice from hundreds of the top scientists inside and outside the government. There was growing concern that Congress had no comparable institutional means of tapping the expert counsel of the scientific community. Congressional uneasiness in this regard was aggravated by the changing composition of the federal civil service. Since 1950 both the number of professionals and the proportion of scientists and engineers had grown rapidly. By 1963, out of the 231,000 or so professional people in the employ of the federal government, about 150,000 were scientists, engineers, or doctors.[6] With few doctors and no scientists among its members, Congress might well wonder how it could equip itself to oversee this aggregation of expertise.

In fact, this whole preoccupation with science and public policy was a local manifestation of a world-wide phenomenon. In Great Britain, in March, 1962, the Prime Minister appointed a Committee of Enquiry into the Organization of Civil Science, which reported to Parliament in October, 1963.[7] The Minister of Science (then Lord Hailsham, now just plain Quintin Hogg, M.P.) published a book Science and Politics, which was circulated in this country in the summer of 1963. And needless to observe, British author C. P. Snow's two booklets,

The Two Cultures and the Scientific Revolution and Science and Government have been widely cited in both high governmental circles and the scientific community in the United States. On October 3 and 4, 1963, the ministers responsible for science in the twenty member governments of the Organization for Economic Cooperation and Development met in Paris to discuss "Science and the Policies of Governments." The problems aired had a familiar ring: the need to establish priorities for allocation of increasingly scarce science resources, how best to structure scientific advice into the governmental process, the necessity for governments to make greater efforts to coordinate their national R & D programs, and so on.[3]

In Great Britain, governmental policies toward science is to be one of the major issues of the forthcoming general election. The Labor Party has been trying to make political capital out of the so-called "brain drain," the migration of scientists to the United States (instead of from the Midwest to California or the East). There is even a British equivalent of the MURA controversy with a lively debate on how government support should be divided between national linear accelerators and the international facilities at CERN in Switzerland. Likewise, Soviet scientists and officials are engaging in dialogues over the relations of science and government.[9]

A distinctively American contribution, however, was made by various congressional committees. Over the immediately preceding years, besides the hearings and studies by the committees on armed services, science and space, appropriations, and atomic energy on scientific programs of agencies within their jurisdictions, several subcommittees of the Senate and House Committees on Government Operations had been energetically studying R & D activities, interagency coordination of scientific information and research, and various aspects of governmental organization for science. In the wake of Sputnik, the Subcommittee on Reorganization and International Organizations of the Senate Government Operations Committee recommended the creation of a Department of Science and Technology. Reflecting a consensus within the scientific community, the Administration wisely rejected this proposal in favor of the present arrangement for a scientific advisory staff at the presidential level. Interestingly, in establishing the Office of Science and Technology, the Administration was following a course recommended by the Subcommittee on National Policy Machinery of the Senate Government Operations

Committee, which the Administration adopted in part as a preclusive measure. Undeterred, the Senate Government Operations Committee Reorganization Subcommittee simply modified its tactics to urge the creation of a Commission on Science and Technology to study the problem. On March 8, 1963, the Senate Government Operations Committee persuaded the Senate to approve a bill (S. 816) to set up such a Commission on Science and Technology, which would be required to report by January 1, 1965, recommendations for:

> A Cabinet-level Department of Science and Technology or transfer of scientific functions to existing or new federal agencies (e.g., consolidation of the Atomic Energy Commission, National Science Foundation and Bureau of Standards).
>
> Improved procedures for conducting or financing scientific programs and supporting basic research.
>
> Elimination of duplication among federal agencies engaged in scientific and technological research and in information and distribution services.[10]

A similar bill passed the Senate in 1962 but was not acted upon by the House. Although the prospects for enacting this measure were not great, the supporting arguments and antecedent bills over the preceding six years had given currency to the idea of extensive waste and duplication in federal R & D activities, accentuated awareness of their rapidly growing cost, and underscored the need for a comprehensive study of all the science and technological programs of the federal government.

Although working without much fanfare, the committee which did the most to alter the climate of relations between science and Congress was the Intergovernmental Relations Subcommittee of the House Government Operations Committee, headed by Representative L. H. Fountain (D.-N.C.). In 1959 the Intergovernmental Relations Subcommittee staff initiated a study of the administration of research grants by the National Institutes of Health. In April, 1961, the subcommittee made a report of its findings, which the full committee adopted and issued as House Report No. 321. Although critical of the organization and procedures of the NIH for administering grant programs, the report was constructive in tone and made reasonable recommendations for correcting extant deficiencies. Moreover,

other recommendations of the subcommittee revealed sympathetic understanding of the problem of reimbursement for indirect costs of research and of the need for developmental grants to build research capability in a larger number of universities and professional schools.[11]

In correspondence during April and June, 1961, Dr. James A. Shannon, Director of NIH, characterized the report as "excellent" and Dr. Luther L. Terry, the Surgeon General, wrote: "Again, may I compliment you upon a searching and instructive inquiry into the growing and complex set of activities administered by the National Institutes of Health."[12]

On August 1, 1961, at hearings of the Intergovernmental Relations Subcommittee on the report, Surgeon General Terry testified:

> Although the Public Health Service activities are constantly under review, there is a tendency for long-established systems and procedures to appear adequate to those familiar with them even when they may no longer meet all of the new requirements imposed upon them by the growth or the changed character of the programs they serve. A critical review by an objective outside group is therefore of great assistance in calling attention to slowly growing but as yet unobserved administrative deficiencies.
>
> It is in this light in which I view and warmly welcome the report's comments on the administration of the NIH grant and award programs. The intelligent examination of recent practices and the thoughtful recommendations for their improvement are very helpful in focusing attention on problem areas and suggesting the need for revised procedures.
>
> I want to assure the committee that each criticism is being most carefully examined and each recommendation most seriously considered both in my office, by the Director of the NIH, and by those immediately responsible for the grant administration at NIH.[13]

On January 22, 1962, in response to a request from the Intergovernmental Relations Subcommittee for a detailed progress report on action taken by the NIH with respect to each of the committee's recommendations in House Report No. 321, Dr. David Price, Acting Director of NIH, wrote:

> We are in accord with the recommendations of your committee and definitely intend eventually to make all desirable changes needed to effect the soundest administration which your committee recommended.[14]

Despite these endorsements of the subcommittee's recommendations, the NIH did virtually nothing to comply with them. Although the ostensible reason for this inaction was the difficulty of recruiting the type of administrative personnel required, a high NIH official subsequently conceded that the NIH had decided to do nothing, saying:

> Fountain is right that nothing was done after the first report. At that time, we had no comprehension of the seriousness of the matter. We had differences among ourselves as to what should be done, and as a result, we did nothing. Some people felt that no changes were needed, and there was a feeling that time would pass and the whole thing would be forgotten.[15]

Meanwhile, in January, 1962, with the assistance of personnel from the GAO, the Intergovernmental Relations Subcommittee staff made an audit of the NIH research grants to Public Service Research, Inc., a commercial firm operating for profit, in order to provide detailed information on the adequacy of NIH policies and procedures for insuring the appropriate expenditures of public funds. This audit covered the period of July 3, 1959-December 31, 1961, during which the company received $378,596 or 85 percent of its total cash funds, from NIH grants, and revealed a number of serious abuses. Conceding that the amount of funds represented only a small percentage of total NIH grants, the staff report stated that NIH policies and management procedures provide no assurance that practices similar to those followed by the Public Service Research, Inc., were not widespread; for the NIH had admitted in hearings that adequate administrative machinery did not exist either in NIH or in the grantee institutions to ensure effective management of grant funds.[16]

On March 28-30, 1962, the Intergovernmental Relations Subcommittee held another hearing "to obtain further information on the progress of the NIH in implementing the committee's recommendations." Then the subcommittee prepared a strongly critical report, which the full committee adopted and issued as House Report No. 1958 on June 30, 1962. Noting that the NIH had acted on some of the committee's recommendations, the report charged that the NIH had done "relatively little to improve the overall management of its grant programs since the committee's report of April 1961." Attributing the weaknesses in the grant programs "in large measure to the failure of NIH officials to understand the nature of their responsibility for the management of public funds," the report went on to say:

> The committee agrees that the selection of good investigators and good projects is vital to productive scientific research, but the effective management of grants is also a fundamental responsibility of a Government agency charged with administering grant programs.
>
> The committee takes strong exception to the view expressed by NIH that all administrative actions subsequent to the selection of grant projects are "essentially trivial" in relation to the basic selection process. The selection process and grant management are essential and complementary parts of NIH research support. Excellence is required of both.[17]

Despite the castigation of the top NIH officials and the angry tone of much of the language, the report stressed:

> The committee wishes to emphasize that it is fully committed to the principle of allowing scientific investigators the greatest possible freedom of action in carrying out their research. The history of science clearly demonstrates that scientific achievement and progress have generally occurred under conditions which allow maximum freedom of inquiry for the investigator.
>
> However, freedom for the scientist should not be confused with license or fiscal irresponsibility . . . .What we must achieve is a harmonizing of freedom for the investigator with responsibility to the public in the expenditure of Government funds. NIH has the obligation to develop adequate policies and procedures for assuring that grant funds are prudently spent within this context.[18]

After observing that the average quality of NIH grant projects according to the NIH's own statistics had been steadily declining in recent years, the report concluded:

> It appears that the Congress has been overzealous in appropriating money for health research. The conclusion is inescapable, from a study of NIH's loose administrative practices, that the pressure for spending increasingly large appropriations has kept NIH from giving adequate attention to basic management problems. <u>The committee expects NIH to give high priority at this time to the task of correcting its management deficiencies and strengthening its capacity for the effective and efficient operation of these vital health programs</u>.[19]

In response to this report, NIH adopted new policy regulations embodying many of the committee's recommendations in October, 1962, and issued the <u>Public Health Service Grants Manual</u> containing them on January 1963. Among the requirements imposed by the new regulations were: scientists who make significant changes in research projects must report this to NIH; any purchase of $1,000 or more with grant money must be approved by NIH; grantees must keep records and report to their institutions the amount of time put in on the federally

financed projects and the institutions must refund NIH money if actual time spent falls short of estimates; and these records are subject to NIH audit.[20]

Although many knowledgeable observers agreed that such PHS regulations were long overdue, the reaction of the scientific community--particularly the medical researchers--was swift and pronounced. Claiming that the regulations burdened researchers with excessive paper work and would cripple scientific inquiry by the restrictions imposed, these critics focused their fire initially on the PHS for having adopted the regulations. As the spring progressed, increasingly they assailed Representative Fountain for having pressured the NIH into precipitately adopting these restrictive administrative practices. The tenor of these attacks is conveyed by the following excerpts from the resolution passed by the American Society of Biological Chemists at its April meeting requesting the National Academy of Sciences to undertake a study of the government's research grant policies:

> The necessity for clearer definition of the relationships involved has been brought into focus by the criticisms recently directed by the Intergovernmental Relations Subcommittee of the House Committee on Government Operations against the management of the research grants program of the United States Public Health Service. While regretting the manner of criticism of the House Committee and their failure to provide constructive leadership, we suggest that the time is indeed opportune and the moment critical for appraisal of the relationships which properly should obtain among the federal government, universities, and scientific investigators if the national interest is to be served.[21]

Besides these direct assaults, a counterattack was launched through sympathetic House committees. In April 1963, the House Interstate and Foreign Commerce Subcommittee on Public Health and Safety, which was headed by Representative Kenneth Roberts (D.-Ala.) began hearings on a bill (H.R. 2410) to give the Surgeon General broad powers to reorganize the PHS. Both Roberts and Representative Oren Harris (D.-Ark.), chairman of the full committee, employed them as a means of making a study of the NIH along with the rest of the PHS. From his sympathetic questioning of NIH Director James Shannon, Roberts appeared to be seeking information with which he and Harris could combat NIH critics. The same month in the report accompanying the appropriations bill (H.R. 5888--House Report No. 246) prepared by the Fogarty Subcommittee, the committee expressed concern at

reports that the efforts of the NIH to tighten fiscal control over the administration of grant programs "seriously threaten the freedom of scientists, and that they constitute evidence of Federal control over science." Then the report went on to say:

> The Committee is sensitive to the fact that overzealous response to criticism might result in unneeded restrictions. However, the Committee is convinced that what has taken place is a needed and proper clarification of the necessary conditions which accompany the use of large sums of public monies. . . . At the same time, the Committee directs the Public Health Service to exercise a high degree of vigilance that its actions not diminish the basic independence and integrity of our institutions of higher learning, and the essential conditions of scientific freedom.[22]

The response of many scientists--particularly the more vociferous--to congressional and agency attempts to introduce some measure of fiscal accountability for the use of research grants was exaggerated to say the least. Implicit in many of the utterances forthcoming from the scientific community was a philosophy that somehow scientists were morally superior to the rest of mankind and that the federal government should supply them money without questioning the uses to which that was put as long as it went for science. Needless to observe, the expression of such sentiments heightened congressional suspicions that there was widespread carelessness in the handling of funds Congress was providing for the support of science. Disclosures by NSF auditors that the American Institute of Biological Sciences had diverted federal research monies to the production of science motion pictures (with the intent of making restitution when the funds were recouped from the sale of the films), the extensive publicity attending difficulties with the MOHOLE Project, and other evidences of mismanagement of federally financed research programs added fuel to the flames.

Yet another ingredient in the quickly heating stew were the acrimonious public disputes among scientists over fallout shelters, environmental contamination, the test ban treaty, and other political issues with a large scientific content--or reciprocally, scientific issues with strong political implications. Most prominent among these was the debate over goals of the U.S. space program. No occasion was too sacred for scientists to profane with their opinionated views on this score, as Linus Pauling's disruption of the ceremonies celebrating the

centennial of the National Academy of Sciences demonstrated. A variety of factors contributed to the intensity of their wrangling. Some scientists were genuinely concerned that a disproportionate amount of scarce resources--particularly scientific manpower which was rapidly approaching a ceiling--would be diverted to the space program while more worthy projects languished. Others, reflecting the biases of their disciplines on such issues as the kinds of data that should be collected, questioned the desirability--much less the feasibility--of the manned lunar landing. Reinforcing these scientific doubts were partisan considerations. Early in 1963, Republicans in both the House and Senate decided to exploit the controversy over the U.S. space program for partisan gain. And the Air Force, disgruntled with its role in space, was eager to supply anyone and everyone with documentation for the criticism that not enough emphasis was being placed on military uses of "inner" space for our national security.

Finally, special mention should be made of the influential role played by Science magazine in general and by its editor, Phillip H. Abelson, in particular. With a circulation of more than 90,000, this house organ of the AAAS reaches most influentials in the scientific establishment and in Washington officialdom. The masthead of Science proclaims the goal of "serving its readers as a forum for the presentation and discussion of important issues related to the advancement of science, including the presentation of minority or conflicting points of view, rather than by publishing only material on which a consensus has been reached." In the area of science and government, Science succeeded admirably in achieving that goal in 1963. The recently expanded and very able staff of the "News and Comment" section provided a comprehensive weekly coverage of significant developments in the "interface of science and politics" on the national and international scene. Free from dictation of an editorial party line, they wrote witty, lucid, perceptive, generally objective but often provocative articles. Supplementing their presentation, as a self-appointed scientific Cassandra, Editor Abelson, penned a series of trenchant editorials plucking at the raveled sleeve of relations between Congress and science. And the feedback to his strictures in the enlarged letters-to-the-editor section stimulated further ntroversy.

On June 6, 1963, Representative Abner W. Sibal (R.-Conn.) introduced a bill to establish a Congressional Science Advisory Staff (H.R. 6866), purportedly at the instance of Dr. John Heller, executive director, New England Institute for Medical Research, and science advisor to Richard M. Nixon during the 1960 presidential campaign. Following the pattern of the Congressional Office of the Legislative Counsel, the bill would establish separate Congressional Science Advisory Staffs for the House and Senate. Each would consist of a director and two associates, none of whom is to be more than 55 years old. Every staff member is to hold a Ph.D. degree or its equivalent, with one from the field of biology, one from chemistry, and one from physics on each staff. Every staff member is to be appointed solely on the basis of fitness and without reference to political affiliation for a term of three years (but shall not be eligible for more than two terms), with compensation of $21,500 a year for each director and $20,000 a year for each associate.

The directors are to be appointed by the President pro tem of the Senate and the Speaker of the House, respectively. The associates are to be appointed from a list provided by each director; in the case of the Senate, one by the President pro tem. and one by the minority leader; and in the case of the House, one by the Speaker and one by the minority leader. The bill provides for clerical staff and, further, that other individuals may be appointed on a temporary basis by the director of each staff to render such scientific assistance as the director may determine to be necessary. These individuals are to be compensated at a rate of up to $100 a day, plus certain expenses.

Each science advisory staff is to be available to the committees and members of the House of Congress within which it is established and to conference committees of the two Houses to give scientific advice and assistance in the analysis, appraisal, and evaluation of legislation or proposed legislation.

On August 13, 1963, Senator E. L. Bartlett (D.-Alaska) introduced an analogous bill to establish a Congressional Office of Science and Technology (S. 2038), to which he refers by the snappy acronym COST. The following day, Representative William Widnall (R.-N.J.) introduced an identical measure in the House (H.R. 8066).

Though generally similar, the Bartlett-Widnall bill improves on the Sibal proposal in several respects. Only the directors of the Senate and House branches of COST are to be appointed by the President pro tem of the Senate and the Speaker of the House, respectively. Again, they are to be selected solely on the ground of fitness and without regard to political affiliation, but no other qualifications are specified. Each director shall employ and fix the salaries of the assistant director and other employees in his office. The bill does, however, list the following duties of the office in each House:

> (1) upon request, to advise and assist any Member or committee of that House with respect to matters relating to science and technology;
>
> (2) to make studies concerning matters relating to science and technology as may be directed by that House or any committee thereof;
>
> (3) to maintain a register of scientific and technological consultants who have indicated a willingness to advise and assist committees and Members of that House; and
>
> (4) to transmit to committees of that House reports concerning significant scientific or technological developments which pertain to matters subject to the jurisdiction of such committees.

The bill provides that each branch of COST shall assist the joint committees upon request at any time. And it provides that should questions of priority be raised concerning requests made to either office, those shall be determined by the Rules and Administration Committee of the Senate and the House Administration Committee in the House.

Though differing in detail, these two bills (Bartlett-Widnall and Sibal) are sufficiently similar that they can be analyzed jointly. Hereafter they will be referred to interchangeably as the COST measure. While well-intentioned, this proposal is defective in several vital respects. Firstly, it is based on a faulty assumption about the nature of policy issues involving scientific matters. These are not scientific questions which can best be answered by a scientist. Almost every policy question involves many other considerations of a political, economic, social, psychological, or military sort. And even the scientific aspects of a policy decision may not be susceptible to precise determination. For example, the decision on ratifying the test ban treaty

was based on weighing foreign policy, domestic political, military, and health factors. And each of these involved imponderables. How would the friendly and neutralist nations react if we refused to negotiate with the Russians? How much would the lessening of tension and possible abatement of the arms race compensate for the military risk of test cessation while the Russians are ahead of us in high yield nuclear weapons? How likely would the Russians be to catch up with us in low yield nuclear weapons by conducting underground tests which the treaty would permit? How would the "Mother" vote react to failure to approve the treaty? None of these is a scientific question.

Secondly, it is based on a misconception of the requirements for a congressional staff member who handles scientific measures. He does not have to be an outstanding scientist in a particular field. Rather, he must be a scientific generalist who can rapidly develop sufficient knowledge of an unfamiliar field to deal intelligently with matters in that area. He must be able not only to translate scientific issues into terms the layman can grasp but also to bridge the growing communications gap among highly specialized branches of science. He performs an interpretive role more analogous to that of the science writer than of the practicing scientist. He must be able to express himself with facility in speaking and writing. He should be thoroughly familiar with our system of government--especially the legislative process. And he must be aware of how science fits into policy decisions. Being a scientist is no guarantee that an individual will possess these qualifications. As Dr. Edward Wenk, Jr., a distinguished graduate engineer who has performed brilliantly in top staff positions in both the legislative and executive branches, incisively observed:

> Second, competence in science or engineering is no guarantee of competence in national political affairs. If anything, the talents, training, and temperaments involved in these two professions are quite different.[23]

In fact, when assessing the political implications of a scientific enterprise, a scientist may be handicapped because of his perspective as a scientist. For instance, many scientists tend to be somewhat contemptuous of space spectaculars, which may have far greater political impact than more solid scientific achievements. Moreover, while a scientific background is helpful, it is not indispensable. A congressional staff member with the other requisites for the post can acquire sufficient expertise to cope with the technical aspects of his work.

Thirdly, it disregards the existing scientific staff capability in the Congress and available means of tapping the expert counsel of the scientific community. Many of the committees dealing with scientific measures have very capable professional staffs--a number of which have had technical training. To mention only two, the House Science and Astronautics Committee has had a core of very competent professional aides from its inception as a select committee. The high quality of the staff studies produced by them is widely recognized. In 1963, there was a first class engineer as well as a former science administrator and science writer on the regular professional staff and two engineers on detail from the armed services. The staff director of the Senate Aeronautical and Space Sciences Committee was a top-notch engineer who had an impressive record of experience as a science administrator--including managing laboratories for the AEC. Another professional staff member, who was a carryover from the antecedent select committee, had a B.S. in aeronautical engineering and a Ph.D. in psychology. In the Legislative Reference Service, the position of senior specialist in science and technology has consistently been filled by a highly competent Ph.D. in engineering or science. Moreover, the senior specialist in engineering and public works and specialists in other fields, like defense and conservation, have supplemented the committee staffs--sometimes for protracted periods--as well as performed assignments for individual Congressmen in the area of science and public policy.

Also, a number of committees have been experimenting with various techniques for obtaining the advice of leading scientists outside Congress. Besides the numerous committees that have employed consultants on an individual basis, a few have contracted for the services of consulting firms like the Rand Corporation. Some have organized panel discussions of experts on specific measures. And in 1960 the House Science and Astronautics Committee established a Panel on Science and Technology with a distinguished membership on a permanent basis. The committee meets periodically with this advisory panel in seminar-like proceedings. In these meetings, the panelists help to acquaint the committee with current views on broad issues in the scientific community. In addition, panel members act as individual consultants to the committee in their respective fields.[24]

There is little likelihood that the COST bill will be approved by the House even--much less enacted. At hearings on the Sibal and Widnall versions (H.R. 6866 and H.R. 3066) before the Subcommittee on Accounts of the House Administration Committee on December 4, 1963, the committee members did not display much enthusiasm toward the proposal. The almost complete lack of questions by them and the meager list of witnesses indicated that the committee was indulging in a formality rather than conducting a serious inquiry into the merits of the measure. The Senate Rules and Administration Committee has not scheduled hearings on the Bartlett bill (S. 2038) and is not likely to do so.

The chief significance of the COST bill is that it stimulated expansion and refinement of the existing arrangements for providing Congress with advice on science-related issues. Besides serving to indicate the growing dimensions of congressional concern about the problem of obtaining scientific counsel, the prospect--however remote--of the adoption of such a defective proposal prompted the search for alternative solutions as a preclusive measure.

In the hearings on testimony of scientists on space goals on June 10-11, 1963 (themselves a highly successful demonstration of how the regular hearings process can be used most effectively to obtain the counsel of nongovernmental scientists), the Senate Aeronautical and Space Sciences Committee asked every witness what he considered to be "the best type of organization and process whereby the legislative branch can obtain the kind of information needed to make decisions on policies, programs, and budgets involving scientific and technological problems." Both the Daddario and Elliott Committees have included this topic on the agendas for their comprehensive investigations of federal research programs. And the ensuing discussions have helped clarify thinking inside and outside Congress about the nature of scientific advice on public policy and the requirements for "in-house" science staff capability for Congress.

Several committees have intensified their efforts to recruit additional staff members with scientific and engineering backgrounds as well as the other qualifications for a professional aide. Encouragement has been given to

developing closer cooperation between committee staffs handling scientific measures in the two chambers. And committees have been considering how to use advisory panels more effectively and experimenting with various mutations of that device.

In the legislative branch appropriations bill for fiscal 1965, the Legislative Reference Service requested eight new positions for specialists in science and technology at relatively high grades (1 GS-17, 2 GS-15, 1 GS-14, 2 GS-13, 1 GS-12, and 1 GS-9) with a price tag of $110,950.00 in order to establish a special unit to handle the work in that area.[25] The House Appropriations Committee approved the whole request. The accompanying report explained this unusual generosity in the following quaint language:

> The committee has rounded off the request, allowing $2,300,000, which will cover mandatory-type costs to maintain the current level and permit addition of 8 new research people to meet the increasing requests for service in the scientific and technological specialties . . . The committee thinks this congressional research assistance arm ought to be reasonably well equipped to meet the demands on it. Increasingly difficult questions in greater abundance are coming before the Congress as the country grows and society complexifies.[26]

On April 10, 1964, the House passed and sent to the Senate the legislative branch appropriations bill for fiscal 1965 with the entire amount for the LRS specialists in science and technology intact.

Also, the process of developing more effective linkage between Congress and the National Academy of Sciences-National Research Council was accelerated. With characteristic foresight, Dr. George B. Kistiakowsky had anticipated the demand for scientific advice for Congress. One of his motives in fostering the establishment of the NAS's Committee on Science and Public Policy (COSPP) more than a year and one half earlier was to help find a way of supplying this need. And the very able and perspicacious executive secretary of COSPP, Robert E. Green, has labored energetically and imaginatively on this task. At the aforementioned hearings on scientists' testimony on space goals, Dr. Frederick Seitz, president of the National Academy of Sciences stated that the NAS-NRC would welcome the opportunity to do more to serve Congress through its permanent

staff and system of part-time consultants. In the hearings before the House Science and Astronautics Subcommittee on Science, Research, and Development on October 15, 1963, Chairman Emilio Daddario (D.-Conn.) explored this offer as the first order of business in questioning Dr. Seitz, the lead-off witness.[27] On January 3, 1964, Representative George Miller (D.-Calif.), chairman of the House Science and Astronautics Committee, announced the reaching of an agreement with the National Academy of Science to advise the Congress on science-related policy issues. Chairman Miller said that he, Dr. Seitz, and Representative Daddario had agreed in principle on the provision of certain services of the National Academy to the committee. These would include the preparation of a series of studies to be worked out between panels of the Academy and the Daddario Subcommittee. Representative Daddario said that this agreement with the Academy was one of several such arrangements which his subcommittee was working out with private and governmental groups to assist the full committee with its activities in the field of science and public policy.[28]

NOTES

The author wishes to express his appreciation to the Purdue Research Foundation for a grant which made possible three months' research on this subject in Washington, D.C., during the summer of 1963. He is most grateful to the following individuals who gave generously of their time and information in protracted interviews: Charles S. Sheldon II and Edward Wenk, Jr., Executive Office of the President; Robert E. Green, National Academy of Sciences; Eilene M. Galloway and C. Edward Wise, Jr., Legislative Reference Service, Library of Congress; Frank C. DiLuzio and Glen P. Wilson, Senate Aeronautical and Space Sciences Committee staff; and W. H. Boone, John A. Carstarphen, Jr., Richard P. Hines, and Philip B. Yeager, House Science and Astronautics Committee staff. Because most of the persons interviewed requested that any information they furnished not be attributed specifically to them, footnote references have not been made to interview sources unless an individual has been directly quoted in the text. Finally, none of these persons nor the Purdue Research Foundation is responsible for the views expressed in this paper.

[1] Government and Science: Their Dynamic Relation in American Democracy (New York: Galaxy Book, Oxford University Press, 1962), pp. v-vi.

[2] The statistics in the preceding two paragraphs are from U.S., Congress, House Committee on Science and Astronautics, Fiscal Trends in Federal Research and Development, committee print, 88th Cong., 2d Sess., Feb. 26, 1964, pp. 3-6.

[3]Congressional Quarterly Weekly Report (cited hereafter as CQ), Vol. 21 (Jan. 18, 1963), p. 52; ibid., (Mar. 15, 1963), p. 326; ibid., (Dec. 6, 1963), p. 2133; Science, Vol. 141 (Sept. 20, 1963), p. 1163.

[4]Science, Vol. 142 (Nov. 22, 1963), p. 1044.

[5]The Reporter, Vol. 29 (Sept. 26, 1963), pp. 20-26.

[6]Figures supplied by Dr. Edward Wenk, Jr., in personal interview on Sept. 5, 1963.

[7]Report of the Committee of Enquiry into the Organisation of Civil Science, Command Paper 2171 (London: Her Majesty's Stationery Office, 1963).

[8]Science, Vol. 142 (Oct. 18, 1963), pp. 372-73.

[9]Science, Vol. 143 (Feb. 21, 1964), pp. 786-88; ibid., (Feb. 28, 1964), p. 919; ibid., (Mar. 13, 1964), pp. 1146-48; New Scientist (Nov. 7, 1963), p. 301; Nature, Vol. 200 (Dec. 21, 1963), p. 1133.

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[11]U.S., Congress, House, Committee on Government Operations, Health Research and Training: The Administration of Grants and Awards by the National Institutes of Health, House Report No. 321, 87th Cong., 1st Sess., Apr. 23, 1961, pp. 72-75.

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[13]U.S. Congress, House, Committee on Government Operations, Hearings on Health Research and Training, 87th Cong., 1st Sess., Aug. 1, 1961, p. 2.

[14]House Report No. 1958, 87th Cong., pp. 10, 42.

[15]As quoted in Science, Vol. 140 (June 7, 1963), p. 1076.

[16]House Report No. 1958, 87th Cong., pp. 11, 50-51.

[17]Ibid., pp. 13-14, 25.

[18]Ibid., p. 21.

[19]Ibid., p. 26.

[20]CQ, Vol. 21 (May 31, 1963), p. 844.

[21]Science, Vol. 140 (May 17, 1963), p. 795.

[22]U.S., Congress, House, Committee on Appropriations, Departments of Labor and Health, Education, and Welfare, and Related Agencies Appropriation Bill, 1964, House Report No. 246 (to accompany H.R. 5888), 88th Cong., 1st Sess., Apr. 25, 1963, pp. 25-26. Also, see CQ, Vol. 21 (May 31, 1963), pp. 845-46, and Science, Vol. 140 (May 3, 1963), pp. 467-68.

[23]U.S., Congress, House, Committee on House Administration, Hearings on Establishment of a Congressional Science Advisory Staff, 88th Cong., 1st Sess., Dec. 4, 1963, p. 68. See also, James L. McCamy, Science and Public Administration (University, Ala.: University of Alabama Press, 1960), pp. 172-73.

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[27]U.S., Congress, House, Committee on Science and Astronautics, Hearings on Government and Science, 88th Cong., 1st Sess., Oct. 15, 1963, pp. 32-33.

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GPO 881-280

Viewpoint of the Scientific Community

W. N. HUBBARD, Jr., M.D.

IT IS POSSIBLE to look upon grants management in its relation to the scientific community as an exercise in bringing to this group a keener awareness of the significance of fiscal accountability for categorical efforts. In this context, the importance of the budget as a fiscal expression of program would be emphasized. The program itself ideally would be a statement of the nature of the effort which the scientist himself wished to undertake. This program would be developed and evaluated on the basis both of its scientific merit and its relevance to the interests of the potential supplier of funds. The budget would then be prepared as a reflection of this program and be altered only as the program itself changed. At the conclusion of the described effort, expenditures within the budget would have been made in such a way that it would be possible retrospectively to confirm that expenditures had been for the originally approved purposes.

It is difficult to imagine why such an obviously reasonable effort should lead to a "crisis of confidence" between those responsible for awarding and managing grants and the scientific community. Rather than discuss the operational technicalities, I should like to describe what I believe to be some of the fundamental reasons for this present crisis.

Meaning and Purpose of Grant

The word "grant" usually has a different meaning for the grantor, the recipient institution, and the individual scientist. The grantor typically has in mind an attack on a defined problem in health.. The recipient institution frequently looks upon the grant as a means of supporting a segment of the much broader program of the total institution. The scientist typically views the grant as a recognition of his personal capability to undertake a scientific effort that has been conceived by him and considers that the grant, in this unique sense, is his.

The congressional intent in appropriating Federal health research funds is to support the resolution of health problems that have been recognized in the people of the United States. These specific health problems are expressed by the names of the principal disease states related to premature death and to impaired capacity to function. Funds are appropriated separately for each category according to the relative importance that it seems to have in the health of the community. However, these categories represent a set of such highly complex phenomena that they cannot actually be approached intact and directly by the scientific method.

One limitation of scientific explanation is that where organized complexity increases, the probability of a satisfactory explanation based on highly certain and reproducible observations which are the foundation of scientific inquiry sharply decreases. Where there are large sets of systematically interdependent variables that are characteristic of living systems, the phenomena that reflect the interaction of these multiple variables must be dealt with by a reductionist approach.

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Reprinted from the "Public Health Reports", a publication of the U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE Public Health Service

Our present mathematics and computer technology cannot deal with the level of complexity that is reflected in a human being. Even such concepts as heart disease, cancer, aging, mental illness, mental retardation, and social delinquency are all phenomena of such great complexity that we must now approach their understanding and scientific study by laboriously reducing them to arbitrary smaller components that are subject to direct examination. It is, of course, possible to attempt to study a single pair of variables within these complex systems, but it is a highly unlikely assumption that definitive causal relationships can thereby be deduced. At the outset then, the complexity of the health problems that are to be solved as opposed to the limited capacity of science as we know it today to deal directly with such problems represents a dilemma.

Aggravating this issue is the observation that when complex phenomena have been reduced to scientifically manageable components, these components typically have no unique or restricted relevance to the original problem. They can be related to the categorical health problem, principally by intuition, common sense, or general observation. As a result, the purpose of studying these components has no more necessary relationship to the function of health from which they are derived than the isolated components of an automobile have to its function of transportation. There thus develops a fundamental logical problem created by the discrepancy between the intent of the research appropriation and the means by which this intent is served by the scientist. Many of the misunderstandings that have arisen between the research managers and the scientific community are related to this logical dilemma.

The nature of the grant must be adapted, then, to provide an accommodation between the parties to this dilemma. Like a happy marriage, the grant is a union of governmental and scientific communities. In a happy marriage, both parties must have a broad area of common purpose and a mutual satisfaction with the fruits of the union. Both parties must be strengthened by the union itself. Neither can allow himself to dominate to the point of imperiling the essential individuality of the other. If the condition of the grant is clearly destructive to the interests of one or the other party, then a dignified separation device that will protect the fruit of the union (scientific knowledge) must be at hand. The union itself must have an important value so that each party is willing to submerge his more selfish interests to the union's purposes and be able to gain satisfaction, in part, through the satisfaction of the partner's needs.

The defined purpose of a grant inevitably confronts the opportunistic nature of creative scientific effort. Here serendipity can have a greater value than the planned programs. The scientist must necessarily view his freedom to depart from preconceived plans as an essential component of his intellectual role. The grantor, however, is necessarily less concerned with this aspect of the scientist's contribution and seeks a continuing effort that has obvious relevance to the initial plans of the research. It is important then to understand that the intent of the grantor, that is, an increase in purposeful or applicable knowledge relevant to a specific human problem, is by nature at odds with the discipline of the scientific method itself.

Concern with the human condition falls outside of logical systems of science although these systems are applicable to a study of phenomena in humans. Although the scientist may be motivated personally by humanistic values and concerns, his actual scientific research does not deal with humanistic purposes. The scientist and the nonscientist have great difficulty in communicating when discussing this research. This difficulty is to be anticipated since to the scientist the fruits of scientific inquiry are their own sufficient justification. In the nonscientist's value system, however, scientific knowledge is justified only when it can be used to alter phenomena that he perceives will affect his own welfare. It is because of this difference in value systems that a good deal of suspicion exists on the part of the nonscientist which leads him to ask for more control of scientific activity as a prerequisite for proper stewardship of public funds.

The grant has been described by Dr. James Shannon, director of the National Institutes of Health, as a "contingent gift." This terminology seems to me to reflect nicely the role of the grant in accommodating the intrinsic differ-

ence between the concepts of the grantor and the actual operative realities of scientific research.

Managerial Authority

Much has been written about the function of the manager in our modern society. A whole new literature has become available in the last two decades dealing with the managerial role. The very concept of a social organization implies the existence of authority and the availability of some decision-making process. Since the scientist requires institutional organization for his efforts, he inevitably becomes involved, albeit against his will and better judgment, in these institutional problems of authority and decision making.

I would assert at the outset that the scientist must be an influential part of the decision-making process if his productivity is not to be inhibited. The managerial role within the organization that includes the scientist will be more effective and influential when the manager works with "the consent of the governed" rather than by decree. It is only by involving the scientist in the authority hierarchy and in the decision-making process that it is possible to engage him in direct responsibility for institutional and social goals as well as his own personal ones. By such engagement, one will achieve much more active participation than could ever be gained through a purely downward movement of authority from some centralized power structure. This is not a novel concept since it is the basis of the strength of the democratic process. In terms of management theory, however, it departs sharply from traditional concepts.

Traditional theories on sources of managerial strength and authority are drawn from industrial, business, governmental agency, and military settings. These institutions are very different social structures than the university, which is the most common social setting for the scientist. It is highly probable that the organizational settings which have given rise to traditional management theories are not suitable for translation into university administration. The idea of purchasing a man's time for an agreed output and then having authority over that time so that the product is assured represents the foundation of traditional managerial stewardship. The basic authority of the manager in such a setting is derived from his control of the economic motivations of the worker. Relatively, the manager is much less concerned with noneconomic motivations such as personal status, self-importance, independence, peer recognition, and general reputation.

Corporations and managers of large enterprises are increasingly recognizing the significance of these noneconomic factors in their personnel practices. In the university setting, however, and for the scientist, these noneconomic factors are frequently of a distinctly high order of importance. In the university, where market prices for professional skills are rarely paid entirely in dollars, those who have managerial authority deal heavily with both noneconomic and economic considerations.

The high loyalty of scientists to their own professional groups must be appreciated. These loyalties may exceed those to his department, school, and university. However, if he is isolated from the institutional setting, his professional identity itself is threatened. In the management of grants, Rensis Likert, director of the Institute for Social Research, University of Michigan, has described the need for "a high level of reciprocal influence." The managerial function is properly conceived, in my opinion, as serving this purpose.

Scientific Community

What is the nature of the scientific community as we know it? I have alluded to the necessity of the scientist working in an institutional setting in order to be effective. The institutions that have historically fostered science most successfully are the universities plus a small number of institutes which have adopted the patterns of the academic tradition. The Rockefeller Institute and the intramural program of the National Institutes of Health in the United States can represent models, while the Pasteur Institute in Paris and the Weizman Institute in Jerusalem would expand the number of examples. Each university and institute is a micro society, created to support the purposes of knowledge—its discovery, dissemination, and preservation. If this basic purpose is not accepted by the larger society, then the

value of these institutions is threatened. The nature of the threat is that the knowledge must have relevance that is recognizable in the terms of the grantor without appropriate consideration of the nature of scientific inquiry. The analogy of the goose and the golden egg is clear.

If research itself is segregated as a function from other university roles, then the nature of the academic community is jeopardized. The institute concept is dependent on the academic tradition of the university. It is not a substitute for such tradition nor does it fit congenially within the greater university setting. It can, however, draw from the great strength of the university tradition so long as its derivative nature is clearly recognized.

Within the university setting, there has been criticism of the increasing amounts of research conducted within traditional academic departments. It is alleged that the education of college students today is deteriorating because of the usurpation of faculty interest by research. If the educational experience really is deteriorating, it is difficult for me to understand the greatly increasing demand for the output of this "degraded" environment.

One of the important components of the university tradition is stability in support of the community of scholars. In this context the scientific community fears the price of rigidity that must be paid if this stable support is to be related to a categorical area of research. The unpredictable component in creativity can be readily smothered by a soothing mass of mediocre effort if accounting for time and effort is allowed to substitute even in part for scientific excellence.

Our present level of knowledge, no matter how effectively distributed, cannot offer a resolution to the health problems that we face today. The critical responsibility in grants management is to resolve the crisis of confidence that has been allowed to develop between the scientific community and those responsible for the award and administration of scientific support. The restitution of confidence will be based on mutual understanding. No set of regulations or elaboration of business and fiscal devices can substitute for this understanding.

Housing for Migrants

The Division of Community Health Services of the Public Health Service and the Agricultural Engineering Research Division of the Agricultural Research Service will provide funds for a joint project to design and test improved housing facilities for migrant farmworkers.

Under the project, present types of migrant housing will be evaluated. Plans will then be developed for the construction of improved low-cost housing designed for short-term occupancy by migrant workers, and prototypes will be erected for testing in various locations having different climates.

The project is designed to result in the provision of improved housing for seasonal farm laborers, particularly in the area of short-term occupancy where, heretofore, economic factors have militated against the provision of healthfully adequate housing. This will require a reexamination of thinking concerning legal restriction governing such housing, since the application of generally accepted minimal standards developed for urban, year-round occupancy housing becomes economically unfeasible in the area of short-term occupancy.

The Competition for Quality

Non-salary factors affecting the selection, recruitment, development, and retention of superior personnel in the scientific service of the Federal Government

FEDERAL COUNCIL FOR SCIENCE AND TECHNOLOGY
April 1962

Reprinted by the
U. S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

FOREWORD

During the past year the Federal Council for Science and Technology has been studying ways to improve the Government's capabilities for conducting and managing its expanding program of research and development. The issue of most serious concern is the recruitment and retention in the public service of scientists and engineers of high professional distinction.

Results of the Council study are contained in its report entitled "The Competition for Quality." The report is in two parts. Part I dealt with the question of salaries for Government scientists and engineers. This document, Part II of the report, makes recommendations on non-salary measures to improve the environment and incentives for research in the Federal Government. The report is based on a detailed study by a panel under the chairmanship of Dr. Allen V. Astin, Director of the National Bureau of Standards. Also included on the panel were Dr. Harve J. Carlson, Dr. Hugh L. Dryden, Dr. Charles V. Kidd, Dr. Richard A. Weiss, and Mr. J. Lee Westrate (Ex Officio).

The Federal Council believes the Government must immediately initiate vigorous remedial measures to maintain the high professional stature of its senior scientists. This requires providing the managerial and professional environment in which scientific competence will thrive. This report recommends specific steps which can be taken immediately to establish employment policies and practices within the Federal Government more hospitable to independent thought and professional attitudes -- an environment which will help to attract and retain gifted and vigorous scientists and engineers, and encourage their initiative and leadership at every echelon of Government. Action on these recommendations will go far to insure excellence within the Federal R&D establishment.

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INTRODUCTION

Science plays a vital role in assuring continued advancement of the Nation's health, welfare, economy, and security. Science activities of the Federal Government are an essential component of the national scientific effort. They require well-trained and competent scientists and engineers to carry them out. Whether Government research programs are conducted in Federal laboratories or by non-Government organizations, Government scientists of the highest competence are needed to: (1) perform research or (2) conceive, plan, direct, and evaluate research done for the Government.

If the United States is to improve or even sustain its current world leadership in science, Federal laboratories must compete more effectively with other employers for scientists and engineers capable of providing imaginative leadership, effective management, and significant achievement.

Present Government salary scales are not competitive for high quality scientists and engineers. Furthermore, fringe benefits now available to Federal employees lag behind those offered by industry. Result: Federal laboratories are in imminent danger of losing many of their best scientists and engineers.

Quality attracts quality, however. One of the compelling attractions for scientists is the opportunity to work with colleagues of outstanding reputation and ability. Thus, providing strong scientific resources for tomorrow begins with hiring promising young scientists today. Once hired, they are more likely to stay if given adequate opportunity for professional recognition and development. A high level of competence, and a sensitivity to the nature and needs of science, must be maintained throughout the Federal establishment -- by managers, supervisors, technical assistants, and supporting service personnel.

Government personnel policy has traditionally favored practices that ensure fair and equitable treatment for all. Too often, this has led to concentration on adequacy rather than excellence, thus failing to provide sufficient incentive or recognition for the superior performance of truly outstanding individuals. In view of the wide diversity of occupational skills, education, and ability now required to staff and to conduct Federal programs, increased flexibility in personnel policies is needed to ensure practices appropriate to the development and retention of superior individuals at all levels of leadership. This is particularly true for scientists and engineers in research and development activities.

Adoption of the recommendations made herein would do much to improve the present situation. The recommendations are consistent with procedures and policies found to be effective in progressive private research organizations.

This paper does not discuss all factors affecting the successful staffing of Federal research and development programs. Attention should be given as well, to the simplification of separation procedures for unproductive employees, to the physical facilities for research, to the training and selection of technical managers and supervisors, to continuity of support, etc. However, to better cope with future changes, it is apparent that two guiding principles should be followed: (1) Federal personnel and salary systems must be made more responsive and administered with maximum flexibility and (2) increased authority must be delegated to technical directors responsible for research and development programs.

I. A FAVORABLE ENVIRONMENT FOR SCIENCE

Scientist Participation in Decision Making

The magnitude and significance of Federal scientific activities demand the highest degree of responsiveness from policy-making officials of departments and agencies employing scientists and engineers and at the U. S. Civil Service Commission. Importantly, scientists and engineers must share in the development and execution of Government personnel policies as they affect the Federal research environment.

The appointment of a Special Assistant to the President for Science and Technology and creation of a President's Science Advisory Committee constituted recognition that the President must be provided with the best advice the scientific community can supply. Equivalent recognition is needed at the Civil Service Commission and in each department and independent agency employing a significant number of scientists and engineers. Generally this would involve having at least one scientist in an influential position at the headquarters policy level. He would perform two important functions: (1) assist top agency and departmental officials in assaying the probable impact and feasibility of major decisions affecting research and development activities, and (2) better interpret to agency scientists and engineers some of the non-science decisions that affect their work.

In addition to helping formulate policy, scientists and engineers must participate in developing administrative procedures relating to Federal research and development activities to assure an optimum environment for research and development. Specifically, they must be given a voice in procedures affecting procurement, personnel, budgetary planning, and security, such as that exercised by the Standing Committee of the Federal Council for Science and Technology. To insure increased participation by Government scientists in directing Federal science activities, we recommend the following:

RECOMMENDATION: The Civil Service Commission is urged to consider ways to obtain continuing expert advice from scientists and engineers and from research and development administrators in all of its considerations affecting the Government's scientific activities.

RECOMMENDATION: Responsibility for providing counsel and advice on overall policies affecting both the research and development programs and personnel environment should be entrusted to a scientist or engineer, unencumbered by any other major duties, in an influential position at the policy level of each department and agency which is a major employer of scientists and engineers.

Technical Responsibility and Administrative Authority

A key element in providing a favorable environment for scientific research and development is the quality of laboratory leadership. An optimum environment for research requires high leadership competence in planning and directing scientific programs and in guiding and inspiring the professional staff. The effectiveness of a well-qualified laboratory director, however, can be largely nullified if he does not have authority commensurate with his responsibilities. Frequently laboratory directors, particularly in some of the defense agencies, do not have adequate control over the administrative service activities which support the primary mission of the laboratory. Often, for example, such a trivial matter as the approval of travel allowances on an actual subsistence basis cannot be made by the laboratory director, but must be referred to the department level.

Service activities, such as library, personnel, fiscal, supply, shops, and plant maintenance are essential to the proper functioning of a laboratory. Their purpose is to facilitate the work of the scientists and engineers in fulfilling the technical responsibilities of the organization. The general nature of laws and regulations under which Government laboratories must operate makes it easy for employees charged with administrative tasks to exercise a degree of control rather than perform service. Such employees must realize that their role is to provide support and assistance to the technical staff. They must understand the objectives, methods, procedures, organizational structures, and other distinguishing characteristics peculiar to research and development organizations and their specialized personnel. They must identify themselves and their activities with the technical objectives of the organization and provide all possible assistance to the technical programs.

This ideal situation will prevail only if the supporting administrative service staff as well as the scientific staff are fully responsible to the laboratory director. In a number of Government laboratories accountability of some of the supporting service units to organizational echelons above the laboratory director denies to him the authority he must exercise to discharge effectively his primary technical responsibilities.

Authority to approve selections for top-level professional and scientific positions should also be decentralized. Decisions relating to qualifications, selections, and appropriate pay levels for professional and scientific positions must be based primarily on the judgments of persons qualified to evaluate the importance and quality of research performed. Within broad guidelines established for the Government service, authority to act on these judgments should be vested in the heads of research and development installations. In contrast, existing practices frequently require a considerable degree of review and subsequent approval by department and agency headquarters officials. Such centralized control tends to delay personnel actions unduly, sometimes with serious effect on recruitment prospects. Negotiations with prospective scientists for key positions must be conditional because of the absence of final authority to take action. Appropriate delegation of authority to approve selections for top positions would help remove the competitive handicap which now impedes heads of Federal research and development establishments in recruiting prospective applicants for critical positions. Such delegation will necessarily be influenced by the size and specialized nature of the operating program and its ability to operate successfully under delegated authority.

RECOMMENDATION: Directors of research and development installations should be given the degree of direct control over administrative service activities required for effective support of the technical mission assigned to the laboratory. This should include, to the maximum extent practicable, the delegation of authority to select and appoint top-level officials in the research installation.

II. OBTAINING QUALITY SCIENTISTS AND ENGINEERS

The Image of the Federal Government as an Employer of Scientists and Engineers

Scientists would be attracted to a Government career by more aggressive and imaginative communication of Federal research opportunities and challenges. The responsibilities of Government laboratories embrace practically all major areas of science and engineering and the programs undertaken cover the spectrum from the most speculative basic research through applied research and development to exacting and critical test and evaluation. Despite the tremendous financial investment of the Federal Government in science and technology and the broad range of significant research conducted in Government laboratories, citizens, including educational leaders, college students, and Congressmen, frequently have little appreciation of the magnitude of Government science and its impact on their daily lives, health, and welfare.

If citizens are largely unaware of the intellectual challenges inherent in many Federal programs, they also know little about many of the material advantages of Government service.

...During recent years Government scientists, as well as other Federal employees, have obtained liberal life insurance and health benefit plans.

...Congress has recognized the value of officially supported training of all types.

...Authorization has been extended scientists to receive reimbursement for travel to their first post of duty.

...In many cases, Government scientists and engineers have been hired at salaries above the normal Federal entrance salary for their grade.

These advances are in addition to such benefits as payment of salary while on military leave for training, liberal sick-leave allowances, and an adequate retirement system. Of prime importance to scientists contemplating Federal employment, however, is the opportunity to pursue long-term research on programs of national significance. The wide diversity of these programs offers exceptional opportunities for the exercise of individual initiative and ingenuity -- a feature that should

be brought more insistently to the attention of the Nation's scientists, particularly at the college level. Although scientist-to-scientist communication is the most effective medium for getting the facts across, more active, imaginative, and informative publicity in the professional and trade press and in the general news media would also represent a contribution to improve recruitment of scientists and engineers.

RECOMMENDATION: The Civil Service Commission is urged to provide increased leadership and assistance, in cooperation with agencies and departments concerned, to communicate to scientists and engineers as potential employees the opportunities and professional challenges offered by science in the public service, and to inform the general public more explicitly of the scope and achievements of Government science and technology. Departments and agencies should also give increased support and attention to this area.

Challenging Program Statements

In an informal survey of Federal scientists' attitudes toward Government employment made by the Interdepartmental Committee on Scientific Research and Development in 1955, the item most frequently mentioned was 'the challenge of the problem.' The work to be done was seen as the Government's major recruiting attraction and strongest holding power. This indicates the importance of stating research goals in clear, challenging, and current terms.

Existing program descriptions or mission statements are frequently outmoded and do not portray the work in terms of its scientific challenge, relevance to national goals, and relation to modern science. Too frequently, routine responsibilities appear to overshadow creative opportunities. Mission statements give meaning to the laboratories' efforts, and direction to their staffs. They should challenge and interest scientists to be recruited. They must be widely disseminated, discussed, and understood both by present and prospective employees.

RECOMMENDATION: To give substance and direction to the competition for quality, Federal research agencies, departments, and laboratories are urged to prepare and publicize program descriptions and mission statements in clear, current, and challenging terms.

Laboratory Recruitment Efforts

With few exceptions, Government recruitment programs for scientists and engineers have been unimaginative, weak, and sporadic. Successful industrial laboratories, on the other hand, put aside a great deal of money and use the time of their best scientists to search for good people.

First-line Government scientists and engineers should be used much more extensively for face-to-face contacts with prospective quality candidates. Scientists typically want to discuss their prospective assignments in specific terms with a working scientist-employer.

Although varying considerably from one Board of Civil Service Examiners to another, present examining procedures are often unduly protracted. The appointing process should be expedited lest the many advantages of personal recruitment are nullified by undue delay in the selection and appointment process.

Systematic recruiting efforts should not be limited to educational institutions. Opportunity should be taken to recruit from industry capable scientists whose jobs may have lost their original appeal, or who may have been caught in layoffs caused by contract cancellations or other economic disturbances. Closer working contact should be established with personnel officers and operating officials of industrial concerns.

In recent years the Civil Service Commission has taken a number of steps to coordinate and assist agency recruitment efforts, including the granting of authority to make on-the-spot commitments to applicants in shortage categories. The Commission has, as well, a full-time recruitment officer in Washington and in each of its regional offices. Agencies are urged to work closely with the Civil Service Commission to make full use of its assistance.

RECOMMENDATION: Federal research administrators are urged to make greater use of their professional scientific staff to support recruitment efforts.

RECOMMENDATION: The Civil Service Commission is urged to give further attention to individual practices and policies of agency Boards of Civil Service Examiners with a view to streamlining and expediting examining and certification procedures.

Reimbursable Travel for Pre-employment Interview

Recruiters of scientists for Government employment are placed at another competitive disadvantage with private industry by the prohibition against reimbursing candidates for interview travel to the Federal facility. American companies frequently make suitable financial arrangements to bring the applicant to the laboratory site where he can be interviewed before a decision on selection is made (often a company requirement). Multiple interviews are widely used to improve the selection process. Similar authority should be granted Government laboratory heads in order that they may interest outstanding candidates in the work of the agency by showing them its facilities and equipment and having them meet their fellow-scientists engaged in the research program. On-site interviews give prospective employees a truer picture of the work and its surroundings and reduce the possibility of subsequent misunderstandings and turnover. Where competition for well-qualified scientists is particularly acute and where Government has many unique and challenging programs and modern equipment and facilities, paid pre-employment travel could be a determining factor in obtaining some of the most sought-after candidates.

RECOMMENDATION: The Special Assistant for Science and Technology and the Bureau of the Budget are urged to support vigorously legislation being prepared by the Civil Service Commission to grant agencies the authority to pay pre-employment interview travel expenses for qualified applicants.

Short- and Long-term Career Opportunities

Government employment opportunities for scientists and engineers should not be restricted to a single type of appointment. Many well-qualified scientists and engineers could undoubtedly be attracted to Government service if they could be hired on a temporary, honorific basis similar to special appointments and fellowships in universities. They would welcome the opportunity to take part in many of the Government's unique and interesting research programs for specified periods ranging perhaps up to five years. Some appointees might later accept career appointments in the Federal service.

Precedents for making non-tenure appointments already exist in the NAS-NRC Postdoctoral Research Associateship

programs at the National Bureau of Standards, the Naval Research Laboratory, and the National Aeronautics and Space Administration. They have resulted in placing exceptionally qualified individuals in Government laboratories to work with career Government scientists on problems of mutual interest. An encouraging number of these excellently qualified persons have chosen Government employment after conclusion of their associateship appointments. This device should be extended to other agencies, should include more senior scientists, and should cover longer periods of appointment.

Other non-tenure devices to supplement and improve Government research staffs include personal services contracts; guest worker and research associate cooperative programs; and, legislation permitting, not only term appointments, but the rotation of term employees between working assignments and educational training. Large numbers of competent research assistants with B.S. and M.S. degrees are needed to facilitate the work of senior scientists and to permit more rapid program progress. Authority to extend term appointments to scientists and engineers at these levels would greatly alleviate this problem. In some instances such authority would need to be accompanied by Government-sponsored training and education to serve as an inducement for the best candidates to compete for the appointments, and, as well, to ensure their most effective utilization.

Existing educational institutions are unable to satisfy the demand for graduate scientists and engineers in a number of scientific fields. Yet in these very fields adequate facilities and uniquely competent scientists are available at various Federal laboratories. If authority were granted to make a substantial number of term appointments and to sponsor job-related training and education for those appointed, the following benefits would accrue:

a. Higher quality output of the existing staff.

b. Increased access to advanced experimental facilities for graduate training and research activities.

c. Increased national supply of scientists and engineers with superior training.

d. Increased quality of permanent staffs of the sponsoring Federal laboratories resulting from the selection for employment of many of the best research appointees.

Such a program would bring in highly competent people because of the attraction of the training and education features of the plan. Persons appointed but not selected for eventual Government employment would be under no stigma, since no commitment for permanent employment is implied in the term appointment. In either event the Nation would benefit from the increase in its trained scientific manpower resources.

RECOMMENDATION: The Civil Service Commission is urged to initiate programs and provide authorization to Federal research and development organizations which would enable them to employ scientists and engineers for periods up to five years without tenure, without necessarily going through the regular appointment process, and with a minimum of administrative clearance. The Commission should also permit agency-sponsored training for non-tenure individuals, when considered appropriate to enhance their professional development.

Annual Leave Allowances for Senior Scientists

Under the terms of the Annual and Sick Leave Act of 1951, employees coming to work for the Government with no previous Federal service (civilian or military) are granted annual leave at the rate of four hours each two weeks or 13 days per year. Although this leave may seem adequate at first glance, it must be used to cover many absences that would not be considered leave in another setting.

This limitation on leave for new employees is particularly detrimental to the recruiting of able senior research scientists -- a group in extremely short supply in Government service. While their total annual leave allowance of 13 days must cover both vacation and other absence, their colleagues at the same grade level, but with longer Government service, receive an annual leave allowance of 20 or 26 days. Yet these senior scientists whom the Government needs to recruit are persons with considerable experience and with liberal privileges and benefits in their present employment. A further irritating provision of the Leave Act forbids the taking of paid annual leave for the first 90 days of employment.

RECOMMENDATION: The Civil Service Commission is urged to study and make recommendations on the best way to provide a more liberal and realistic annual leave policy for senior personnel accepting Government employment for the first time.

III. INCENTIVES FOR SCIENCE CAREERS IN GOVERNMENT

Dual Appointment and Promotion Opportunities

The value of competent, respected research and development administrators is obvious and is reflected in Civil Service Commission classification standards. However, the value of creative and capable research scientists or engineers to the successful accomplishment of research programs is equally important and needs to be more fully reflected in classification standards and in agency appointment and promotion policies and practices.

The need to assume administrative duties to qualify for senior positions is frequently a source of difficulty in recruiting and retaining superior research scientists. Some agencies meet this problem by establishing dual career development ladders: one based on program and supervisory responsibilities, the other based solely on recognition of difficult and creative research performance coupled with professional qualifications. Civil Service Commission policies now encourage this approach. The opportunity to obtain appointment and promotion strictly on the basis of one's research effort and professional qualification is an important career consideration. It needs to be more fully exploited by Government agencies in recruiting for senior positions, from inside or outside the Government service.

RECOMMENDATION: In promoting and recruiting for senior positions, research directors and the Civil Service Commission should give vigorous publicity to career opportunities available on the basis of research performance and professional qualifications and which carry no management responsibilities.

Flexible Working Schedules

Government working schedule regulations are for the most part derived from the concept of a rigid 40-hour week based on five 8-hour days, with leave being charged for any time taken off during the formal work week.

For clerical and custodial employees, most administrative employees, and a large proportion of engineering and other professional personnel, this system works well. But for

many scientists and engineers, particularly those engaged in research, development, and testing work, a regular 8-hour day and 40-hour week schedule is practically impossible. The nature of such work may not readily coincide with formal, repetitive starting and stopping times. A scientist who works 12 hours in one day because an experiment could not be completed by 5 o'clock, should not be penalized if he comes in one hour later the following day. This special nature of research, development, and special project work needs to be recognized and accommodated by a less rigid approach to the matter of working schedules.

RECOMMENDATION: The Civil Service Commission and the Bureau of the Budget are urged to develop appropriate standards and to provide the heads of research and development laboratories with a greater degree of autonomy in setting flexible working hours for scientists and engineers engaged in research or special projects.

Relocation Assistance for Transferred Employees

Government scientists and engineers, whose talents may be critically needed at a different geographical location, often refuse to transfer because of the financial sacrifices involved. Thus, the Government's own major manpower resource is reduced in adaptability and value because of the inability to provide adequate relocation assistance to its employees.

The American Management Association recently reported on industrial practices concerning the relocation of personnel. According to this report, it is common industry practice to provide some or all of the following services to transferred employees:

a. Complete reimbursement for the moving of household goods and personal effects.

b. Assistance in buying and selling a house, short-term loans, and payment of a percentage of the employee's annual salary to cover miscellaneous relocation expenses.

c. Reimbursement to cover travel costs of the family while en route and while searching for a residence at the new location.

The Federal Government allows its transferred employees no per diem while locating living quarters at the new location. It pays for the transportation of the employee's dependents,

but not for any of their living costs, such as room or meals, even while en route. The Government places a maximum limit on the amount of reimbursement it will allow for transporting household goods. It provides no substantive assistance to an employee in locating, buying, selling, and financing his home, nor does it reimburse him in any way for the many miscellaneous costs and fees.

A precedent for remedying this condition exists in the case of military personnel, foreign service personnel, and other civilian employees assigned to duty overseas. Such employees of the Federal Government already receive reimbursement to cover much of their relocation expenses.

RECOMMENDATION: It is urged that authority be obtained to provide more adequate reimbursement to transferred employees. As a minimum, reimbursement should include: per diem for the employee and his family for a reasonable period of time after they arrive, in which to find new housing; per diem for members of the family while en route to the new official station; and a flat payment based on a percentage of annual salary, to cover the numerous expenses incurred in relocating one's family and residence in a new geographical location.

Participation in Professional Activities Including Attendance at Meetings

Since passage of the Government Employees' Training Act in 1958 Federal agencies have actively supported training programs designed to increase the competence of Government scientists and engineers. Other activities that enhance both the development and the reputation of Federal scientists and engineers include the authorship of books, occasional teaching and lecturing, and participation in professional society activities. While the importance of these activities is generally recognized, administrative policies and support for the attendance of Government scientists and engineers at professional meetings vary considerably from agency to agency.

In May 1954, a panel of the Interdepartmental Committee on Scientific Research and Development made a series of recommendations for a uniform minimum travel policy for Government agencies to apply in authorizing attendance at meetings of learned societies. These recommendations are still suitable for use as guidelines for a constructive agency policy. They state that "on the average each scientist or professional person who is actively associated with research and development activities /should/ be

authorized to attend at least one national meeting of a learned American society per fiscal year at full Government expense." The panel also suggested the following administrative guidelines for use in developing the policy:

"(a) Administrators should recognize the need for encouraging the younger members of their staffs to attend scientific meetings as a means to further develop their competence.

(b) The presentation of a scientific paper at the meeting, while encouraged, should not be a condition for attendance at Government expense.

(c) Scientists who have material worthy of presentation at national meetings of learned societies, and scientists who hold important offices in such societies should be permitted to attend additional meetings at full Government expense.

(d) It is but natural that the national meetings of any given scientific society will appeal almost exclusively to scientists of a particular discipline.... No rigid limitations should be set up that prescribe the maximum number of scientists from a given installation who attend a particular national meeting."

Guidelines of this kind are needed because of the importance and difficulty in developing a policy covering paid attendance at professional meetings. The attendance of a scientist or engineer at the right meetings helps his professional development may bring acquisition of technical news of an immediately and directly useful type, and is a source of personal professional satisfaction. On the other hand, there is a proliferation of meetings of varying quality and significance, and the laboratory administrator must ensure a proper balance between the benefits of attendance that are likely to accrue to Government programs versus the possibility of undue travel and attendance at meetings of marginal value.

The guidelines quoted do not cover attendance at meetings outside the country. In recognition of the broad scope and complexity of science today and the fact that no one nation possesses more than a fraction of the total knowledge available, agency policies should also cover attendance of its research personnel at international meetings appropriate to their function.

An Interagency Advisory Group Committee is currently working with the Civil Service Commission to develop guidelines for sponsoring attendance of employees at professional meetings.

RECOMMENDATION: The Civil Service Commission is urged to issue Government-wide policy guidelines promoting a prudent and permissive Federal attitude toward official travel and attendance of scientific personnel at local, national, and international professional and scientific meetings. Agency and department officials are urged to establish or maintain a similar policy for research laboratory personnel.

IV. SUMMARY OF RECOMMENDATIONS

Subject	Report Page	Recommendation	Action Required
I. A FAVORABLE ENVIRONMENT FOR SCIENCE			
-Scientist Participation in Decision Making	3	The Civil Service Commission is urged to consider ways to obtain continuing expert advice from scientists and engineers and from research and development administrators in all of its considerations affecting the Government's scientific activities.	Civil Service Commission
	4	Responsibility for providing counsel and advice on overall policies affecting both the research and development programs and personnel environment should be entrusted to a scientist or engineer, unencumbered by any other major duties, in an influential position at the policy level of each department and agency which is a major employer of scientists and engineers.	Heads of agencies and departments
-Technical Responsibility and Administrative Authority	5	Directors of research and development installations should be given the degree of direct control over administrative service activities required for effective support of the technical mission assigned to the laboratory. This should include, to the maximum extent possible the delegation of authority to select and appoint top-level officials in the research installation.	Heads of agencies and departments

Subject	Report	Recommendation	Action Required
II. OBTAINING QUALITY SCIENTISTS AND ENGINEERS			
-The Image of the Federal Government as an Employer of Scientists and Engineers	7	The Civil Service Commission is urged to provide increased leadership and assistance, in cooperation with agencies and departments concerned, to communicate to scientists and engineers as potential employees the opportunities and professional challenges offered by science in the public service, and to inform the general public more explicitly of the scope and achievements of Government science and technology. Departments and agencies should also give increased support and attention to this area.	Civil Service Commission and heads of agencies, departments, and research and development installations
-Challenging Program Statements	7	To give substance and direction to the competition for quality, Federal research agencies, departments, and laboratories are urged to prepare and publicize program descriptions and mission statements in clear, current, and challenging terms.	Heads of agencies, departments, and research and development installations
-Laboratory Recruitment Efforts	8	Federal research administrators are urged to make greater use of their professional scientific staff to support recruitment efforts.	Heads of research laboratories and higher officials responsible for agency recruitment programs
		The Civil Service Commission is urged to give further attention to individual practices and policies of agency Boards of Civil Service Examiners with a view to streamlining and expediting examining and certification procedures.	Civil Service Commission

Subject	Report Page	Recommendation	Action Required
-Reimbursable Travel for Pre-employment Interview	9	The Special Assistant for Science and Technology and the Bureau of the Budget are urged to support vigorously legislation being prepared by the Civil Service Commission to grant agencies the authority to pay pre-employment interview travel expenses for qualified applicants.	Special Assistant for Science and Technology and the Bureau of the Budget
-Short- and Long-term Career Opportunities	11	The Civil Service Commission is urged to initiate programs and provide authorization to Federal research and development organizations which would enable them to employ scientists and engineers for periods up to five years without tenure, without necessarily going through the regular appointment process, and with a minimum of administrative clearance. The Commission should also permit agency-sponsored training for non-tenure individuals, when considered appropriate to enhance their professional development.	Civil Service Commission
-Annual Leave Allowances for Senior Scientists	11	The Civil Service Commission is urged to study and make recommendations on the best way to provide a more liberal and realistic annual leave policy for senior personnel accepting Government employment for the first time.	Civil Service Commission
III. INCENTIVES FOR SCIENCE CAREERS IN GOVERNMENT			
-Dual Appointment and Promotion Opportunities	12	In promoting and recruiting for senior positions, research directors and the Civil Service Commission should give vigorous publicity to career opportunities available on the basis of research performance and professional qualifications and which carry no management responsibilities.	Civil Service Commission and heads of research and development installations

Subject	Report Page	Recommendation	Action Required
- Flexible Working Schedules	13	The Civil Service Commission and the Bureau of the Budget are urged to develop appropriate standards and to provide the heads of research and development laboratories with a greater degree of autonomy in setting flexible working hours for scientists and engineers engaged in research or special projects.	Civil Service Commission and Bureau of the Budget
-Relocation Assistance for Transferred Employees	14	It is urged that authority be obtained to provide more adequate reimbursement to transferred employees. As a minimum, reimbursement should include: per diem for the employee and his family for a reasonable period of time after they arrive, in which to find new housing; per diem for members of the family while en route to the new official station; and a flat payment based on a percentage of annual salary, to cover the numerous expenses incurred in relocating one's family and residence in a new geographical location.	Bureau of theBudget and Civil Service Commission
-Participation in Professional Activities Including Attendance at Meetings	16	The Civil Service Commission is urged to issue Government wide policy guidelines promoting a prudent and permissive Federal attitude toward official travel and attendance of scientific meetings. Agency and department officials are urged to establish or maintain a similar policy for research laboratory personnel.	Civil Service Commission and heads of agencies and departments

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Acknowledgements

NIH gratefully acknowledges the cooperation of authors and publishers whose articles, identified in the "Reading List," we have been given permission to reprint. We thank the staff of the Army Management School for their courtesy and assistance in again making available to us their excellent facilities. Lastly, we recognize the valuable contribution of past participants in this seminar. Their thoughtful suggestions have been of considerable use in planning the present agenda.

Sponsorship

The Seminar in Science in Public Policy is sponsored by the Committee on Staff Training--Extramural Programs, an advisory body to the Associate Director for Extramural Programs, National Institutes of Health. The purpose is to improve understanding among the NIH extramural staff of issues, relationships, and influences that surround scientific programs in the federal government. Members of the Committee are:

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